Do Collusive Norms Maximize Profits? Evidence From A Vegetable Market Experiment in India

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Abstract

Social norms have been shown to facilitate anti-competitive behavior in decentralized markets. We demonstrate these norms can also reduce aggregate profits. First, we present descriptive evidence of competition-suppressing norms in Kolkata vegetable markets. We then report on a market-level experiment in which we induced a temporary relaxation of these norms by subsidizing some vendors to sell additional produce. Our intervention raised profits at the market level by over 60%, excluding the subsidy. Nevertheless, after the subsidy ended vendors largely stopped selling the additional produce. Our results suggest anti-competitive norms may partially explain the pervasiveness of small-scale firms in developing countries.

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1 Introduction

Even in the absence of explicit coordination, norms can sustain non-competitive behavior in large, decentralized markets. For instance, Breza et al. (2019) demonstrates that workers in rural Indian labor markets abide by strong norms not to accept job offers below the prevailing market wage and provides similar evidence for workers in urban labor stands, taxi drivers, food vendors, and butchers in India and Kenya. While economic theory offers a ready explanation for how atomistic agents who interact repeatedly can sustain deviations from perfect competition (e.g. Aumann et al., 1995; Fudenberg et al., 2009), the folk-theorem logic does not make strong predictions about whether these deviations raise joint profits relative to the competitive benchmark. Whether these norms serve to raise the incomes of the market participants is therefore an empirical question.

We demonstrate that within vegetable markets in Kolkata, India, anti-competitive norms significantly reduce vendor profits, both at the individual and market level. Our argument has three parts. First, we use a census of fruit vendors in Delhi to document features common to informal markets in the developing world: many small-scale entrepreneurs operate in close proximity to one another, set prices as non-trivial markups over procurement cost, and have spare capacity. These facts suggest that expansion might be feasible. Second, we replicate a finding of Breza et al. (2019) within our experimental vegetable markets by documenting the existence of norms that suppress competition amongst vegetable vendors. 40% of vendors say that other vendors would react negatively if a vendor were to sell produce at 10% below the market price. 45% say that negative consequences would be likely if a vendor expanded their business, doubling the business over a few months, and 27% of vendors say that negative consequences would likely follow if a vendor who had never sold carrots or peas began to sell these products.

Third, and most importantly, we report on a non-randomized experiment in which we induce a temporary violation of these norms and increase market competition. In 20 Kolkata vegetable markets we subsidized vendors to expand their product offerings and utilize some of their spare capacity. We recorded prices and quantities for all vendors in all markets for three weeks. Then, in three markets we offered vendors three-week subsidies to procure and stock carrots and peas. We offered the carrot subsidy to all vendors and the pea subsidy only to those vendors who were not previously frequent pea sellers. The status quo prevailed in the remaining 17 markets. Following the removal of the subsidy, we recorded prices and quantities in all markets for a final two weeks. We use a difference-in-differences approach to estimate the contemporaneous and persistent effects of the subsidies, and for small-N inference we use the wild bootstrap and permutation tests.

Our first finding is that vendors who received the subsidies stocked more peas and carrots during the subsidy period. Vendors tended to sell their expanded stock without cutting prices. As a result, the profits of treated vendors rose by more than 60%, not including the value of the subsidy. Importantly, vendors who received the subsidy had to provide the additional capital up-front and had to procure the additional produce on their own; they were only reimbursed for their purchases later in the day. Hence, by design, all vendors who exploited the opportunity to sell additional peas and carrots must have had access to the capital and knowledge necessary to do so. That vendors can, in partial equilibrium, significantly increase their profits by increasing their inventory is an important finding in its own right, given that there are over five million street vendors in India alone.

Strikingly, after the subsidy period concluded, treated vendors reduced pea and carrot procurement almost to pre-intervention levels. That is, despite having experienced higher profits when they stocked the new products, and despite having the knowledge, capital, skill, and labor required to do so, most vendors reverted to their prior scale of operation and refrained from exploiting the profitable opportunity that they had just experienced.

Could it be that despite having experienced higher profits from expanding their offerings, vendors failed to realize that their profits increased? We view this as unlikely, as this is a case where it is straightforward to verify that profits have increased. As long as revenues from sales of a product exceed the cost of procurement, and as long as vendors have spare capacity to stock the additional products – two features satisfied by our environment – then stocking the additional products increases total profits. For a vendor to verify that he sold peas or carrots at a profit does not require complex counterfactual reasoning. We also show evidence inconsistent with risk aversion and loss aversion explaining the results: at the vendor-by-week level, stocking the additional peas and carrots results in lower profits less than 1% of the time, greatly reducing the specters of risk and loss. Finally, we argue that the sheer size of the effect renders disutility of labor required to expand a vendor's business an implausible explaination for why vendors do not exploit this opportunity.

In summary, we demonstrate the existence of strong anti-competitive norms. We then induce a violation of these norms by subsidizing the sale of additional peas and carrots, thereby increasing vendor profits by 60% on average. Critically our intervention increased profits *at the market level*. We demonstrate that vendors can exploit this deviation without external intervention, and that despite having experienced the additional profits, almost all vendors revert to their initial scale of operation immediately upon the removal of our subsidy. We conclude that these norms not only suppress competition but also aggregate profits.

One limitation of our study is that we cannot definitively explain why our intervention permitted a temporary relaxation of the market's anti-competitive norms. But it is important to note that within treatment markets, it was public knowledge that our research team was providing subsidies for expansion, it was known that these were temporary subsidies, the subsidies were large – intended to cover the full cost of procurement – and the carrot subsidy was extended to all vendors within treatment markets. All these facts contribute to the plausibility that our intervention allowed vendors to temporarily violate market norms.

Our results relate to several literatures. First, recent experiments have characterized market structure in developing countries, finding competitive markets in some settings (Casaburi and Reed 2022) and collusive markets in others (Bergquist and Dinerstein 2020). Unlike ours, these experiments do not test directly for the possibility of profitable deviations to status-quo business practices, and thus cannot show directly whether the existing market structure and associated behaviors maximize joint or individual profits. Our paper then builds on the literature studying collusion in markets by showing that collusion need not maximize market-level profits.

Second, our results relate to broader work on social norms influencing behavior (Bursztyn and Jensen 2015, 2017). In some settings, people misperceive the norm (Bursztyn et al. 2020a, 2023), leading to inefficiently norm-constrained behavior. One interpretation of our results is that, while vendors do not misperceive the norm and the associated social sanctions, they may have incorrect beliefs about the ultimate effects of the norm – i.e. erroneously believing that it maximizes joint profits.

Related to our market setting, Breza et al. (2019) shows that social norms change the aggregate labor supply curve in rural India – workers are unwilling to accept wage cuts when their decision-making is observable to others, and workers are willing to punish those that accept wage cuts, even when such punishment is personally costly. Breza et al. (2019) finds that the norm benefits workers, by increasing worker surplus relative to an equilibrium with no wage floor. We find instead that the collusive norm in vegetable markets *hurts* vendors, by keeping profits low. With this finding, we also relate to work on potentially inefficient or mismatched cultural norms, like female genital cutting, dueling, and footbinding (Gulesci et al. 2021; Nunn 2022).

Third, our results speak to a large literature on the predominance of small firms in developing countries (Lewis, 1954; Hsieh and Olken, 2014). Economists typically focus on input constraints as the key barriers to growth, such as lack of capital (De Mel et al., 2008), labor (De Mel et al., 2019; Hardy and McCasland, 2023), managerial skill (Drexler et al., 2014), and information (Hanna et al., 2014). In our setting, growth is feasible. Microentrepreneurs have an opportunity to materially increase their profits – by over 60% – for which they have access to the necessary capital, labor, skill, and information, and the opportunity poses little additional risk, yet it remains unexploited. A collusive norm, and its associated social sanctions, is the key constraint on scale.

Finally, our paper is related to the literature that documents the failure of some firms to maximize profits. Cho and Rust (2010), Atkin et al. (2017), and DellaVigna and Gentzkow (2019) document various failures of profit maximization due to the organizational complexity of large firms. In contrast, the microenterprises we study are overwhelmingly sole proprietorships. Beaman et al. (2014) and Gertler et al. (2022) document failures by small and microenterprises to adopt business practices that increase profits by 3 to 8%. These authors attribute the phenomena to limits of attention, memory and trust. In contrast, we identify a failure to adopt business practices that are far more profitable as a percentage of baseline earnings, in a context where failures of attention, memory and trust are not plausible explanations.[1]

2 Spatial Competition of Fruit Vendors in South Delhi

We first document several facts about the spatial competition of produce markets, using a census of fruit vendors in South Delhi. These facts serve two goals. First, they demonstrate that typical features of informal markets in developing countries hold in Indian produce markets – i.e. many small-scale vendors operate in close proximity, and many vendors appear to have plenty of time in hand. Second, they suggest that vendors might have scope to expand their scale. In the following section, we present descriptive evidence that collusive norms might prevent such expansion among vegetable vendors in Kolkata.

We conducted the census from November 2018 to February 2019, surveying all contiguous neighborhoods in a 135 square kilometer area. Our census included 1,179 street vendors and 309 vendors operating in designated weekly markets. We asked each vendor to carry out a 15-minute survey covering demographics, fruit variety, daily profits and revenues, bargaining,

[1] Our paper also relates to the literature examining the extent to which micro and small business growth comes at the expense of competing businesses (De Mel et al., 2008; McKenzie and Woodruff, 2008; Drexler et al., 2014; Cai and Szeidl, 2022). Most closely related is McKenzie and Puerto (2021), which examines the impact of business training on female vendors in rural markets. In that setting, providing training to some entrepreneurs did not negatively impact competitors; rather profits increased at the market level. Similarly, we find that our intervention causes profits to increase at the market level, and we find no evidence of negative spillovers on vendors who did not receive the pea subsidy.

and fruit-level questions on procurement costs, selling prices, and quantities. 80% of vendors consented to the survey. For those that did not consent, we nevertheless have their locations geo-referenced and data on the fruits they were selling based on surveyor observation. We use the census to document four facts.

Fact 1: Vendors exhibit a high degree of spatial clustering

Figure 1 maps the universe of vendors in our census area (see Figure A1 for our survey catchment area). Vendors have on average 3.8 other vendors selling fruit within a 25-meter radius, and 1.1 within a 10-meter radius. Furthermore, 27% of vendors have at least one other vendor selling fruit within a five-meter radius. Density is higher for weekly market vendors than street vendors. For example, while 64% of market vendors have at least one other vendor within a ten-meter radius, only 43% of street vendors face such competition.

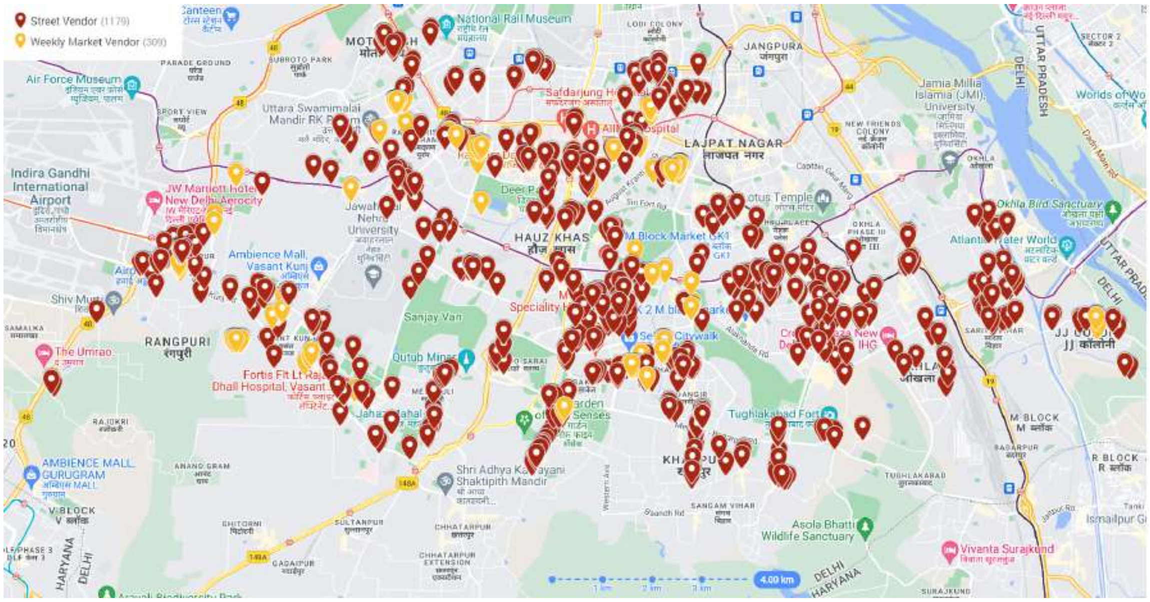
The high density of fruit vendors suggests that a given vendor could scale by attempting to acquire the market share of their nearby competitors. In addition, high density suggests that expansion (which may violate prevailing norms) would be directly observed by neighboring vendors (unlike in Green and Porter (1984) and the related literature where vendors infer collusion from market outcomes).

Fact 2: Vendors charge non-trivial markups over their marginal cost

Despite operating in close proximity, vendors charge meaningful markups over their procurement costs. Using almost 5,000 vendor-fruit-level observations, we find that the average markup is 29%, measured as the stated selling price of the fruit less the stated procurement cost, as a fraction of the stated procurement cost. After accounting for vendors' expectations about discounts given to customers, markups are still 21% on average. Figure A2 plots the distribution of markups in our data.

While it is difficult to gauge whether these markups are big or small in an absolute sense, they are sufficiently large that vendors have room to lower their prices to undercut their com-

Figure 1: Fruit Vendor Locations



Notes: The figure denotes the exact locations of 1,179 street vendors (red) and 309 weekly market vendors (yellow). The figure includes vendors that did not consent to answer the census survey. The rectangular area includes some locations outside of our census catchment area. For the exact catchment area, see Figure A1.

petitors, if they so desired. Nevertheless, though pre-subsidy period markups are similar for vegetable vendors in Kolkata (29% on average for carrots, 23% on average for peas), we will see below that our experimental vendors expanded their inventory and sales without lowering their prices.

Fact 3: A large fraction of vendors' time is spent sitting idly

As part of our census, we asked how many customers vendors served at various times throughout the day for each day of a typical week. Vendors report a large range of typical customers per hour, with Saturdays busier than weekdays on average, and evenings the busiest, followed by mornings and then afternoons. When considering the maximum typical customers per hour across all three slots, vendors still report a large range, with a median of 15 customers per hour, and a 95th percentile of 42 customers per hour. Figure A3 plots the distribution of customers per hour in our data.

Assuming that all vendors have the capacity to operate at the capacity of the 95th percentile vendor, this suggests that even at their busiest hours the median vendor is operating at less than half capacity. Most vendors can then increase their scale without needing to hire employees.

To complement the fruit vendor data, we asked a more direct question on utilized capacity to the 391 vegetable vendors in our descriptive survey in 2023 (described below). We ask "For how many minutes of your typical working hour do you spend waiting for customers (e.g. sitting idly, chatting with friends nearby, etc.)?" Vendors report the time spent waiting as 24 minutes on average, again consistent with the idea that vendors have substantial underutilized capacity. In addition, our experimental evidence below shows that expansion is feasible in practice: vegetable vendors in Kolkata manage to expand their operations without hiring employees.

Fact 4: Nearby vendors maintain a significant degree of product differentiation

Despite a high degree of spatial clustering amongst fruit vendors, the degree of clustering at the fruit-level is considerably smaller. Specifically, averaging over all vendor-fruit-level observations, for any given fruit a vendor sells there is 1 other vendor selling the same fruit within a 25-meter radius, and 0.3 within a ten-meter radius. For a given fruit that a vendor sells, in only 10% of cases is another vendor selling the same fruit within a five-meter radius. In other words, while fruit vendors are often stationed in close proximity, they are much less often selling identical fruits.

Together these four facts suggest that expansion may be feasible: vendors have the spare capacity and opportunity to expand their inventory, potentially lower their prices, and increase their sales volume. Our vegetable market experiment demonstrates that expansion is feasible and profitable in practice. But first we turn to descriptive evidence that collusive norms might hinder such expansion.

3 Collusive Norms in Kolkata Vegetable Markets

Our experiment took place in 20 dedicated vegetable markets in Kolkata. These markets are densely populated; the average market in our study has 83 vendors. In contrast, Bergquist and Dinerstein (2020) finds evidence of collusion in a setting in which the median market has only three vendors. While, as in the Delhi census, there is some differentiation between the vegetables that our study vendors sell, there is significant overlap in inventory. For instance, at the time of our 2018 experiment, 61% of vendors in these markets sold peas and 54% sold carrots – the two vegetables that are the focus of our study. Vendors earned an average profit of Rs. 497 per day (USD 7.1 as of December 1, 2018).

While these markets may not seem conducive to explicit coordination and collusion (i.e. there are many decentralized actors, none of whom seem to have deep enough pockets to punish potential deviators), vendors have on average been in their markets for 24 years. This creates the possibility that strong norms could guide behavior away from perfect competition.

We conducted a survey in October 2023 to investigate the prevalence of anti-competitive norms.[2] We aimed to survey a random sample of 20 vendors in each market, ultimately reaching 391 completed surveys.[3]

A substantial share of vendors confirmed that such norms exist. When asked how a vendor would be perceived if they were to sell a vegetable at 10% below the market price, 30% of vendors described this behavior as unacceptable or highly unacceptable (as opposed to acceptable or highly acceptable), and 40% said that negative consequences would be somewhat or highly likely. Among the vendors that report a negative consequence, the most common are that vendors will spread information about the behavior to other vendors/markets (21%), that other vendors will be angry (18%), that other vendors will prevent them from working at the market (17%), and that other vendors will steer customers away from them (15%).[4]

[2]This survey took place after our experimental intervention in the same markets. The results of the experiment prompted us to explore the existence of collusive norms.

[3]See Appendix A for the full list of our survey questions and summary statistics.

[4]Far fewer report social sanctions of stopping talking (8%) or stopping drinking/playing cards/visiting their

When we ask about a vendor that "worked hard to expand their business, stocking more produce, and almost doubling their business in size over a few months," 14% describe the behavior as unacceptable or highly unacceptable, and a far larger 45% say that negative consequences would be somewhat or highly likely. Finally, 18% of vendors report that it would be unacceptable or highly for a vendor who had never sold carrots or peas before to begin selling carrots or peas, and 27% say that negative consequences of such behavior would be somewhat or highly likely.[5] These findings are consistent with Breza et al. (2019), which documents the existence of similar norms across urban labor stands, taxi drivers, food vendors, and butchers in India and Kenya.

Are these norms well-calibrated to maximize group profits? The most basic theory of explicit collusion would suggest yes, as agents set prices and quantities at the monopoly level (Tirole 1988; Bergquist and Dinerstein 2020). On the other hand, a folk theorem logic predicts that a group of decentralized actors can sustain many behaviors in equilibrium, some of which may be far from profit maximizing (Aumann et al. 1995; Fudenberg et al. 2009). Further, social norms develop over long periods of time and may be slow to adapt to changing circumstances (Bursztyn et al. 2020a; Nunn 2022; Gulesci et al. 2021). Hence, even if these norms were conducive to joint-profit maximization at one point in time, the same may no longer be true today. Or it may be that these norms do still serve to sustain collusion, but are necessarily rigid and simple and therefore prevent the adoption of some profitable opportunities.

Whether these norms maximize group profits is therefore an empirical question. In the next section we describe an experiment meant to induce a relaxation of these norms to investigate their impact on vendors' profits.

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home (3%), and only 3% reported that other vendors would stop lending them money.

[5] For each of the three behaviors we asked about, more vendors said that negative consequences would likely follow compared to the number of vendors who said the behavior was unacceptable. This might reflect that on average, vendors hold incorrect beliefs about how many other vendors would disapprove of these behaviors (Bursztyn et al. 2020a), though we did not ask about this directly.

4 Experimentally Inducing Vegetable Vendors to Increase Scale

Overview. We designed our subsidy intervention to induce vendors to expand their operations, giving them cover to (temporarily) break the prevailing norm. By estimating effects on profits excluding the subsidy, we can then test for whether the norm maximized or constrained market-level profits.[6]

Timeline and Market Selection. Our experiment took place from December 2018 to March 2019 in the same 20 Kolkata vegetable markets described in the previous section. Due to the cost of market-wide subsidy interventions we could only intervene in three markets. With so few treated units, we did not randomize. Instead, we chose three markets with two criteria in mind. First, we chose markets of roughly medium size when compared with all 20 markets. Second, we chose markets with relatively little price volatility for peas and carrots. This reduces the possibility of idiosyncratic market-level shocks confounding the subsidy intervention.[7] Our three intervention markets are Charu Market ($n = 45$ vendors), Sarkar Bazar ($n = 73$), and Alam Bazar ($n = 85$).[8] Figure A4 presents a map of our treatment and control markets.

We break our analysis into three periods: pre-subsidy, subsidy, and post-subsidy. The pre-subsidy period lasted three weeks from December 15, 2018 to January 4, 2019; the subsidy period lasted three weeks from February 23, 2019 to March 15, 2019; and, the post-subsidy period lasted two weeks from March 16, 2019 to March 31, 2019. In each period we collected

[6] Following a rich experimental literature on social norms and social image (e.g. Bursztyn and Jensen 2015; Bursztyn et al. 2020b), an alternative approach would be to randomize the visibility of some vendors' actions to be more private than the status quo. In principle, this would permit those vendors to break the norm without fear of social sanction, leading them to increase their scale, and potentially their profits. In practice, it is not feasible in the market setting to make the actions of some vendors private, given that their interactions with customers are necessarily visible to nearby vendors. In particular, it is not possible to make the actions of vendors more private without introducing additional confounds (e.g. having them sell in a different place with fewer onlookers).

[7] Given that there are more markets in our control group than our treatment group, idiosyncratic variation in our outcome variables is more likely to average out in our control group.

[8] Table A1 presents some descriptive statistics on each of our three intervention markets and our 17 control markets. Our intervention markets had 67 vendors on average while our control markets had an average of 85 vendors. Vendors in our intervention markets earned an average of Rs.355/day compared to vendors in control markets with an average daily profit of Rs.520/day. 57% (50%) of vendors in our intervention markets sold peas (carrots), while the corresponding number in control markets is 61% (54%).

daily data from all vendors in all 20 markets. The data includes the quantity of all vegetables procured each morning, the quantity sold during the previous day, and the sale price and procurement cost of each vegetable. Given our non-randomized approach, we use this panel data for a difference-in-differences strategy, checking throughout that pre-subsidy period trends of key outcomes are parallel.

Subsidy Intervention. The 17 control markets received no intervention during any of the periods. In the three intervention markets we offered a subsidy to all vendors during the subsidy period to procure carrots. The subsidy took the form of a cash payment delivered to vendors each morning if they had procured carrots that day. The subsidy value was equal to Rs.20/kg, which was the median procurement cost of carrots during the pre-subsidy period. The maximum quantity subsidized was randomized at the vendor-level each week to be either low or high. Vendors who received the low subsidy were compensated for a maximum of 2kg of carrots, while the high quantity was set at the median of the distribution of daily wholesale purchases of carrots during the pre-subsidy period, for each market (7kg in Charu market, and 5kg in Sarkar Bazar and Alam Bazar).

While we offered the carrot subsidy to all vendors in intervention markets, we only offered the pea subsidy to infrequent pea sellers – the 40% of vendors who sold peas in fewer than eight of the days during the pre-subsidy period. The pea subsidy value was equal to Rs.30/kg, which was the median procurement cost of peas during the pre-subsidy period, and once again the subsidized volume was either low or high. Like carrots, the low quantity was set at 2kg, and the high quantity was set at the median of the distribution of daily wholesale purchases of peas during the pre-subsidy period, for each market (8kg in Charu market, 6kg in Sarkar Bazar, and 10kg Alam Bazar).

The weekly randomization of subsidized quantity was intended to investigate whether vendors who were offered an opportunity to stock more of the new produce would more persistently adopt these products in the post-subsidy period. Unfortunately our intervention did not induce enough variation in the number of weeks a vendor was exposed to the high subsidies, and thus this analysis is under-powered. Therefore we pool vendors who received a high versus low subsidy and focus only on the market-level variation in whether vendors were offered the subsidy.

We introduced one universal (carrots) and one non-universal (peas) subsidy for two reasons. First, we wanted to ensure that all vendors in intervention markets received at least one subsidy to minimize the likelihood of vendors feeling they were treated unfairly. Second, the two different subsidies allow us to explore the effects of two margins of vendor expansion. The pea subsidy effectively stimulates the "entry" of new vendors who previously did not sell the product and allows us to explore business stealing or other spillover effects on incumbent vendors. In contrast, the carrot subsidy also induces incumbents to expand their inventory on the intensive margin, which illuminates whether vendors are effectively constraining the supply of even the goods they have chosen to sell.

Our design has several features that plausibly induced vendors to violate the norms documented above. First, the subsidies are large enough to fully cover the cost of procuring the additional produce in the median case. Second, the subsidies were publicly known, so all vendors understood the reason for potential norms violations. Third, the subsidies were known to be temporary, lasting only three weeks. Finally, all vendors received the subsidy to stock additional carrots, thus no vendor exploiting these opportunities could be perceived as "leaving the others out." While we do not know the precise mechanism that enabled vendors to violate market norms temporarily, our intervention did succeed in causing a temporary violation.

Empirical Approach

We estimate the following specification:

$$y_{imt} = \alpha + \beta_1 During_t + \beta_2 Post_t + \beta_3 Treat_m + \gamma_1 During_t \times Treat_m + \gamma_2 Post_t \times Treat_m + \varepsilon_{imt} \quad (1)$$

where y_{imt} is the outcome of interest for vendor i in market m on day t. $During_t$ is a dummy taking a value of one if day t was during the subsidy period, $Post_t$ is a dummy taking a value of one if day t was after the subsidy period, and $Treat_m$ is a dummy taking a value of one if market m is one of the three intervention markets. This is a difference-in-differences model where our coefficients of interest are γ_1, capturing the effect of our subsidies during the subsidy period, and γ_2, capturing the persistent effect of our subsidies after the subsidy period had ended.

Because we only have 20 markets with three treated, traditional econometric inference based on large-sample asymptotics is unlikely to perform well in our setting. Instead, we report p-values and confidence intervals computed using the wild bootstrap (Cameron et al., 2008; Roodman et al., 2019), and p-values computed using Fisher's permutation test (Fisher, 1936; Young, 2019),[9] both using markets as the relevant cluster unit. While our tables report the results from both inference approaches, the two approaches largely coincide. Given this, we report the wild bootstrap estimates in the text, and note the permutation test p-values only when the two methods differ in statistical significance at conventional levels.

[9] With 20 markets, there are 1,140 possible combinations of three intervention markets. For the permutation test, we re-run a given regression 1,140 times, each time using a different unique combination of hypothetical intervention markets. We then calculate p-values as the fraction of t-statistics larger in magnitude than the t-statistic from the original regression.

5 Results

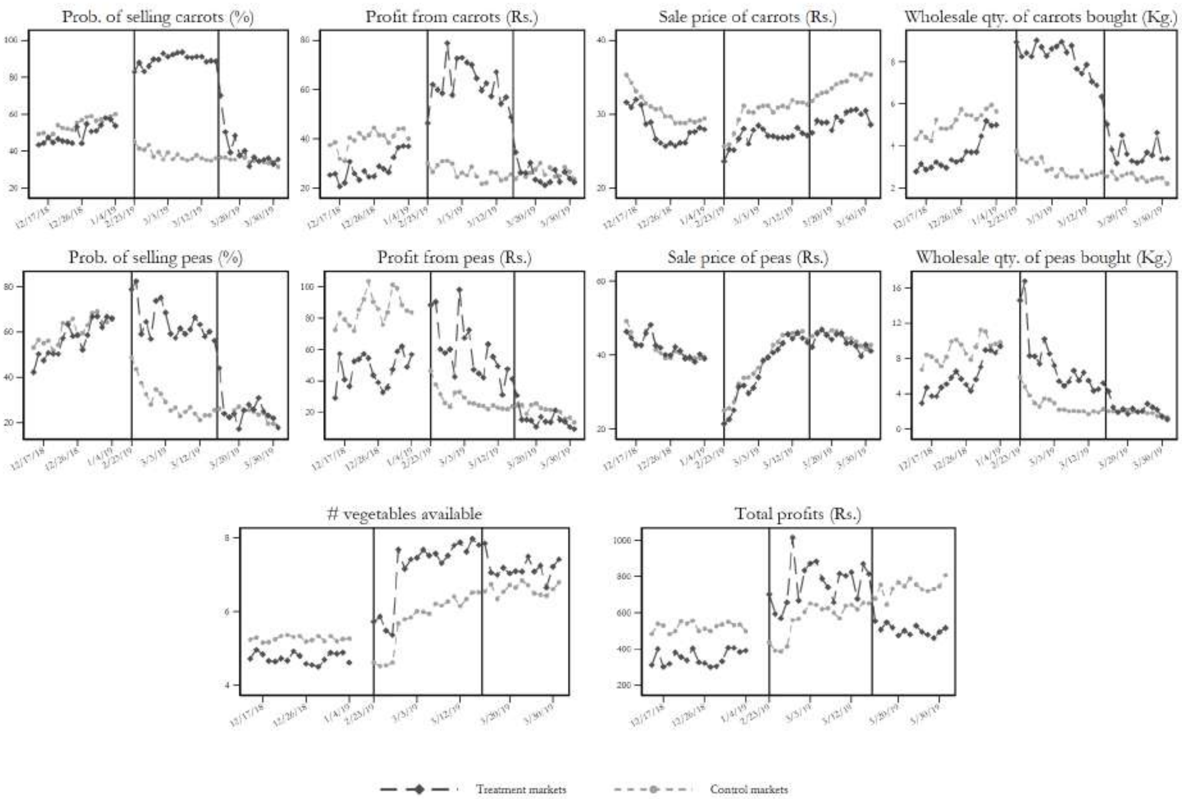

Figure 2: The first row plots the probability a vendor sells carrots, the daily profits accruing from the sales of carrots, the vendor's sale price of carrots, and the quantity of carrots procured in kilograms. The second row plots the same four outcomes for peas. The third row plots the number of types of vegetables a vendor stocks on a given day, and the vendor's daily total profits. The first vertical line demarcates the start of the subsidy period and the second line demarcates the end of the subsidy period. Profits are calculated as: (amount of vegetable at the start of the day - amount left over at the end of the day)* sale price - (amount procured at the start of the day * procurement cost). On days where the amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Graphical Summary. Figure 2 summarizes our main findings graphically. First, outcomes in the intervention markets trend similarly to those in the control markets in the pre-subsidy period. We never reject the null that the trends are parallel (Table A2). Second, the subsidies had an important effect during the subsidy period. Vendors in intervention markets were more likely to sell peas and carrots and had higher average profits from the sales of peas and carrots. They also had higher overall profits during the subsidy period. In contrast, prices in intervention markets trended similarly to those in control markets. Third, the effects of our subsidies largely disappeared in the post-subsidy period.

Before turning to the regression results, we note that the figures appear to exhibit a discontinuity for many of the outcomes in control markets between the pre-subsidy and subsidy periods. This is because one and a half months elapsed between our pre-subsidy period and our subsidy period, and it reflects that the aggregate sales of both peas and carrots declined somewhat in that intervening period due to seasonal variation. Nevertheless, neither vegetable went "out of season" during our study period, with at least 20% of vendors selling each vegetable at all points throughout the study.

The Subsidy Period. During the subsidy period, vendors in intervention markets were 57 percentage points more likely to sell carrots on any given day (95% CI: 40pp – 73pp, $\hat{\gamma}_1$ in column 1, Table 1) and 39 percentage points more likely to sell peas (95% CI: 16pp – 64pp, column 5). On average vendors in intervention markets procured an extra 6.0kg of carrots per day (95% CI: 3.8kg – 7.7kg, column 3) and an extra 6.7kg of peas (95% CI: 4.0kg – 10.0kg, column 7). These are substantial increases relative to average procurement volumes of 3.4kg of carrots and 5.9kg of peas in intervention markets in the pre-subsidy period.[10]

Importantly, profits for vendors in intervention markets increased during the subsidy period, even after subtracting the value of their subsidy. Profits from carrots increased by Rs.44.8 per day (95% CI: Rs.21.1 – Rs.60.1, column 4) compared to an average profit from carrots of Rs.25.4 per day in the pre-subsidy period. Profits from peas increased by Rs.59.7 per day (95% CI: Rs.10.2 – Rs.98.0, column 8) compared to an average profit from peas of Rs.47.7 per day in

[10] Indeed, these point estimates suggest that vendors increased their average procurement of peas and carrots by more than the average subsidized quantity. This may be because once a vendor is induced to procure any positive quantity of peas or carrots (or induced to continue procuring peas or carrots if they would otherwise have ceased doing so), they find it worthwhile to procure more than the subsidized quantity. This would be reasonable behavior, for example, if they have negotiated a temporary exemption from a collusive norm during the experiment, and want to take full advantage of it.

the pre-subsidy period.[11]

In addition, there is no evidence that our intervention caused sale prices for peas and carrots to decline (columns 2 and 6), indicating that vendors had not been meeting customers' full demand prior to our intervention. Thus vendors can expand profitably without reducing prices.

After the Subsidy Ended. The impacts of the subsidy diminished or fully disappeared after the subsidy period ended. There is no statistically significant increase in the likelihood that vendors in intervention markets sell additional carrots or peas ($\hat{\gamma}_2$ in columns 1 and 5, Table 1). Vendors in intervention markets only procured an additional 1.9kg of carrots per day (95% CI: -0.2kg – 4.0kg and only procured an additional 2.6kg of peas (95% CI: -0.9kg – 6.5kg). These are higher than the preperiod but a roughly two-thirds drop relative to the subsidy period. And additional profits from selling carrots and peas fell even farther: profits from carrots were only Rs.8.8 higher per day (95% CI: Rs.-14.1 – Rs.35.0) and Rs.26.5 higher per day for peas (95% CI: Rs.-19.6 – Rs.66.9). All of these figures are statistically significantly lower than the corresponding estimates during the subsidy period.

[11] It is theoretically possible that short and long-term profits diverge. If customers engaged in intertemporal substitution, by buying more peas and carrots than they could eat during the subsidy period and storing them, they may have reduced demand in the following weeks. Then, even though our intervention increased short-run profits, it may have come at the expense of an offsetting decline in long-run profits. This would not be an issue for vendors that had not previously sold peas or carrots. Moreover, the typical shelf life of peas and carrots is between 3 to 7 days, so this is not a likely phenomenon in general.

Table 1: Subsidy Impacts: Carrots and Peas

	Carrot				Peas			
	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_3 Treat	–0.05	–2.62	–1.40	–12.71	–0.04	–0.01	–2.53	–33.29
	[–0.287, 0.191]	[–6.699, 3.012]	[–3.615, 1.010]	[–26.549, 7.743]	[–0.319, 0.264]	[–3.260, 2.661]	[–7.632, 1.997]	[–82.570, 26.028]
	{ 0.651 }	{ 0.086 }	{ 0.303 }	{ 0.307 }	{ 0.563 }	{ 0.986 }	{ 0.089 }	{ 0.082 }
	⟨ 0.602 ⟩	⟨ 0.147 ⟩	⟨ 0.264 ⟩	⟨ 0.141 ⟩	⟨ 0.604 ⟩	⟨ 0.989 ⟩	⟨ 0.147 ⟩	⟨ 0.049 ⟩
γ_1 Treat × During Subs	0.57	–0.44	5.99	44.75	0.39	–0.81	6.73	59.67
	[0.398, 0.725]	[–3.122, 2.035]	[3.758, 7.725]	[21.061, 60.080]	[0.160, 0.644]	[–4.005, 2.755]	[3.956, 10.007]	[10.170, 97.985]
	{ 0.002 }	{ 0.617 }	{ < 0.001 }	{ 0.002 }	{ 0.018 }	{ 0.757 }	{ 0.016 }	{ 0.039 }
	⟨ < 0.001 ⟩	⟨ 0.685 ⟩	⟨ < 0.001 ⟩	⟨ < 0.001 ⟩	⟨ < 0.001 ⟩	⟨ 0.686 ⟩	⟨ < 0.001 ⟩	⟨ 0.002 ⟩
γ_2 Treat × After Subs	0.10	–2.04	1.94	8.79	0.05	–0.87	2.58	26.52
	[–0.075, 0.273]	[–7.815, 5.228]	[–0.162, 3.975]	[–14.129, 34.962]	[–0.187, 0.274]	[–4.614, 2.027]	[–0.936, 6.475]	[–18.532, 63.079]
	{ 0.410 }	{ 0.160 }	{ 0.069 }	{ 0.245 }	{ 0.453 }	{ 0.586 }	{ 0.063 }	{ 0.075 }
	⟨ 0.354 ⟩	⟨ 0.187 ⟩	⟨ 0.192 ⟩	⟨ 0.200 ⟩	⟨ 0.546 ⟩	⟨ 0.532 ⟩	⟨ 0.068 ⟩	⟨ 0.055 ⟩
Pre-subsidy intervention market mean	0.490	27.914	3.433	25.407	0.569	41.798	5.939	47.727
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.311	0.001	0.001	0.003	0.965	0.004	0.009
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.357	<0.001	<0.001	<0.001	0.976	0.002	0.023
Number of Vendors	1631	1470	1631	1631	1631	1489	1631	1631
Number of Observations	55218	25073	55218	55213	55243	22657	55243	55241

Notes: This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in { }, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for carrots, and 5 - 8 for peas. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

We note that 100% of vendors in intervention markets who sold carrots experienced positive profits from sales of carrots during the subsidy period. The analogous number for peas was 99%. Hence these results are not driven by the possibility that a majority of vendors found it marginally unprofitable to sell peas and carrots and only those who experienced the profit increase continued selling peas and carrots. Rather, many vendors who experienced positive profits from sales of peas and carrots nevertheless chose to stop selling these vegetables after our intervention concluded.

Beyond Carrots and Peas. Table 2 presents the impact of our subsidies on aggregate vendor outcomes, rather than those corresponding to either carrots or peas. The aggregate picture is largely consistent with the results from the individual vegetables. During the subsidy period, total costs of wholesale purchases in intervention markets rose by Rs.690 per day (95% CI: Rs.234 – Rs.1,149, column 1) compared to an average cost of wholesale purchases of Rs.825 in intervention markets in the pre-subsidy period. Average vendor profits rose by Rs.228 per day (95% CI: Rs.-52 – Rs.531, column 3) compared to an average profit of Rs.342 in the pre-subsidy period. This is our primary result: inducing vendors to sell more peas and carrots caused an increase in average profits *at the market level*, so it is not merely the case that profits for some deviating vendors rose at the expense of total market profits (as would be predicted if quantities has previously been constrained to monopoly levels). On average vendors stocked an additional 2.0 (95% CI: 0.5 – 3.4, column 4) types of vegetables during the subsidy period compared to an average of 4.7 products stocked per vendor in the pre-subsidy period. Once again, these effects either diminish or disappear after our subsidy concluded, with no statistically significant increase on any of the aforementioned outcomes.

Table 2: Subsidy Impacts: Vendor-Level Outcomes

	Aggregate			
	Total Cost of Wholesale Purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
	(1)	(2)	(3)	(4)
β_3 Treat	–448.49	–589.02	–140.53	–0.50
	[–1073.389, 58.919]	[–1203.300, 46.070]	[–385.185, 114.969]	[–2.525, 1.784]
	{ 0.058 }	{ 0.052 }	{ 0.128 }	{ 0.547 }
	⟨ 0.032 ⟩	⟨ 0.030 ⟩	⟨ 0.075 ⟩	⟨ 0.471 ⟩
γ_1 Treat × During Subs	689.60	917.98	228.32	1.97
	[234.192, 1149.070]	[275.331, 1496.020]	[–52.334, 530.698]	[0.506, 3.388]
	{ 0.027 }	{ 0.033 }	{ 0.066 }	{ 0.031 }
	⟨ < 0.001 ⟩	⟨ 0.005 ⟩	⟨ 0.025 ⟩	⟨ 0.012 ⟩
γ_2 Treat × After Subs	527.12	466.27	–60.84	1.16
	[–126.081, 1079.258]	[–303.151, 1240.917]	[–414.986, 250.636]	[–0.552, 2.651]
	{ 0.266 }	{ 0.322 }	{ 0.325 }	{ 0.309 }
	⟨ 0.268 ⟩	⟨ 0.342 ⟩	⟨ 0.403 ⟩	⟨ 0.228 ⟩
Pre-subsidy intervention market mean	825.121	1167.304	342.183	4.733
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	0.573	0.166	0.033	0.060
Fisher p-value: $\gamma_1 = \gamma_2$	0.582	0.139	<0.001	0.059
Number of Vendors	1628	1628	1628	1628
Number of Observations	52898	52898	52898	52898

This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

Interestingly, the effect of our intervention on total profits is larger than the sum of the effects on the profits from sales of peas and carrots. This difference is only statistically significant at the 10% level when using the wild bootstrap, and not statistically significant at the 10% level using the Fisher permutation test. Similarly, the effect on the cost of total wholesale purchases is larger than the sum of the effect on the costs of purchases of peas and carrots. This difference is statistically significant at the 10% level using both the wild bootstrap and Fisher permutation tests. Hence these results leave open the possibility that our subsidy "crowded in" the sale of complementary produce. One possible explanation is that the subsidy provided cover for vendors to break the norm of non-expansion by also procuring more of the non-subsidized produce.

Pea-Subsidy Impacts by Eligibility. Recall that while everyone in intervention markets received a carrot subsidy, only infrequent peas sellers were eligible for the pea subsidy. We now turn to the differential effects of the pea subsidy on vendors in intervention markets who did and did not receive the subsidy to focus on the extensive margin. These are presented in Table 3, which again uses Specification 1, but now splits the sample by pea subsidy eligibility.

Table 3: Subsidy Impacts: By Pea Subsidy Eligibility

	Eligible				Ineligible			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
β_3 Treat	–0.03	0.38	–0.10	–17.39	0.01	–0.05	–2.87	–37.71
	[–0.062, 0.035]	[–9.990, 14.333]	[–2.347, 0.354]	[–50.080, 17.587]	[–0.113, 0.120]	[–3.125, 3.090]	[–9.707, 3.420]	[–96.298, 41.895]
	{ 0.114 }	{ 0.835 }	{ 0.081 }	{ 0.098 }	{ 0.755 }	{ 0.944 }	{ 0.319 }	{ 0.086 }
	⟨ 0.053 ⟩	⟨ 0.908 ⟩	⟨ 0.060 ⟩	⟨ 0.033 ⟩	⟨ 0.793 ⟩	⟨ 0.961 ⟩	⟨ 0.267 ⟩	⟨ 0.104 ⟩
γ_1 Treat × During Subs	0.67	4.35	7.84	65.59	0.16	–1.996	5.37	50.90
	[0.538, 0.743]	[0.652, 9.503]	[4.120, 10.338]	[32.976, 96.680]	[–0.084, 0.483]	[–5.239, 1.326]	[0.993, 11.342]	[–24.166, 106.831]
	{ <0.001 }	{ 0.036 }	{ <0.001 }	{ 0.004 }	{ 0.074 }	{ 0.282 }	{ 0.038 }	{ 0.075 }
	⟨ 0.002 ⟩	⟨ 0.181 ⟩	⟨ 0.005 ⟩	⟨ 0.008 ⟩	⟨ 0.124 ⟩	⟨ 0.311 ⟩	⟨ 0.007 ⟩	⟨ 0.018 ⟩
γ_2 Treat × After Subs	0.10	0.25	1.74	19.36	–0.00	–0.56	2.66	27.51
	[–0.008, 0.219]	[–3.815, 3.286]	[0.264, 2.838]	[–14.929, 52.739]	[–0.168, 0.223]	[–4.875, 2.785]	[–3.304, 7.159]	[–41.816, 85.503]
	{ 0.064 }	{ 0.909 }	{ 0.039 }	{ 0.108 }	{ 0.952 }	{ 0.798 }	{ 0.249 }	{ 0.175 }
	⟨ 0.120 ⟩	⟨ 0.913 ⟩	⟨ 0.010 ⟩	⟨ 0.013 ⟩	⟨ 0.969 ⟩	⟨ 0.754 ⟩	⟨ 0.274 ⟩	⟨ 0.250 ⟩
Pre-subsidy intervention market mean	0.176	39.381	1.635	10.564	0.847	42.154	8.982	73.993
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.257	<0.001	0.001	0.027	0.611	0.043	0.045
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.539	0.006	0.008	0.025	0.554	0.043	0.060
Number of Vendors	562	480	562	562	1069	1009	1069	1069
Number of Observations	19763	3687	19763	19761	35480	18970	35480	35480

This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

The qualitative patterns for eligible pea vendors are the same as in the previous analyses. During the subsidy period, eligible vendors in intervention markets were 66 percentage points (95% CI: 54pp – 74pp) more likely to stock peas on any given day during the subsidy period, they procured an extra 7.8kg of peas per day (95% CI: 4.1kg – 10.3kg), and earned an extra Rs.65.6 per day (95% CI: Rs.33.0 – Rs.96.7) from the sale of peas. Unlike in the previous analysis, there is evidence of a price increase during the subsidy period of Rs.4.4/kg (95% CI: Rs.0.7 – Rs.9.5), statistically significant at the 5% level using the wild bootstrap, but not when using the permutation test. Qualitative evidence we collected suggests this may be because vendors substituted towards higher quality peas. Once again, all of these effects diminish considerably after our subsidy was removed.

We find no evidence of business stealing effects. In fact, the patterns for vendors who were ineligible for the pea subsidy are largely the same as the patterns for eligible vendors. These vendors procured more peas and earned higher profits from the sale of peas (and higher profits overall, not reported in the table) during the subsidy period, despite not having access to a pea subsidy. Qualitative evidence we collected after the intervention suggests that this is due to informal arrangements between vendors who sold peas prior to our intervention, typically larger vendors, and vendors who did not. Namely, these larger vendors would procure and transport additional peas at the wholesale market and then sell them to vendors who received a subsidy. Note however that this remains consistent with our basic narrative. It is possible for many vendors to increase their sales volume and profits by purchasing and selling more carrots and peas, but even after directly verifying and experiencing these opportunities firsthand, they refrained from exploiting them after our intervention. In addition, the 'sharing' of the pea subsidy is consistent with anti-competitive norms preventing vendors unilaterally getting ahead.

For completeness, in Appendix Table A3 we present analogous results from the carrot subsidy, disaggregated by vendors who were frequent or infrequent carrot sellers in the pre-subsidy period. The results are qualitatively the same. Both types of vendors experienced an increase in sales and profits of carrots during the subsidy period, and then these increments largely disappeared in the post period.

Therefore, on the extensive margin, subsidizing the entrance of new pea vendors increased their profits while also increasing the average profits of incumbent pea vendors. On the intensive margin, inducing existing carrot vendors to expand their supply, while also inducing the entry of new carrot vendors, increased the profits of both groups. In both cases, the additional sales and profits largely dissipated after our intervention concluded.

Having described our core results, we consider two threats to our finding that our treatment substantially increased profits during the subsidy period: spillovers from treatment to control markets, and mismeasurement of profits.

Accounting For Potential Spillovers From Treatment to Control Markets. An identifying assumption of our difference-in-differences approach is that our intervention in treatment markets did not influence outcomes in our control markets. This assumption would be violated if there were demand-side spillovers, whereby vendors who expanded their scale in treated markets diverted customers from control markets, or supply-side spillovers, whereby vendors in treated markets purchased so many peas and carrots from wholesale markets that procurement prices for control vendors increased, reducing their ultimate scale of operation.

To rule out demand-side spillovers, we re-estimate Specification 1 after excluding the control markets that are most likely to be affected by our intervention in treatment markets. Specifically, within each market m we asked all vendors which nearby market customers would be most likely to shop from if they were not to shop at market m. For each treatment market m, we drop any market from our sample that is in the top three markets that are most frequently listed as likely competitors. This results in dropping only three markets from the analysis, as most of the frequently listed competitor markets are not within our experimental sample of 20 markets.

To rule out supply-side spillovers, we re-estimate Specification 1, but remove from the analysis the control vendors whose supply of produce is most likely to be affected by our intervention. Specifically, for each vendor, we know the primary wholesale market from which they procure their produce. 44.3% of vendors in our treatment markets primarily procure their produce from two wholesale markets, while only 7.0% of vendors in our treatment markets procure their produce from the next most common wholesale markets. We drop from our analysis all control-market vendors served by these two wholesale markets, which results in removing 716 vendors.

The results are presented in Tables A4 and A5 for demand-side spillovers, and Tables A6 and A7 for supply-side spillovers. Importantly, none of the patterns are qualitatively altered. Vendors are significantly more likely to sell peas and carrots during the subsidy period and earn significantly higher profits from the sale of peas and carrots, as well as total profits. Again, all of these effects diminish significantly after the subsidy is removed.

Measurement Error in Profits. To compute profits from a particular vegetable we multiply a vendor's sale price by the volume of the good sold, and subtract the procurement price multiplied by the volume of the good procured. One concern is that our estimated treatment effects on profits are biased upward due to mismeasurement of sale prices. In particular, we measure a vegetable's sale price only at one point in the day (i.e. at the day-vendor-level), rather than for each separate transaction. This mismeasurement would lead us to overestimate the profits of treated vendors if (i) treated vendors stay later in the retail market to sell their additional produce, and (ii) treated vendors lower prices later in the day, with our team more often measuring prices earlier in the day, before they have fallen. Against this, we find no evidence of markups falling throughout the day, using variation in the time of day at which a surveyor measured prices (Figure A5).

A second limitation of our profit measure is its omission of several important factors in a vendor's profits, such as the cost of renting a spot in a market, the cost of transporting vegetables from the wholesale market to the market stall, the cost of hired labor, and the opportunity cost of the vendor's own labor. Nevertheless, under reasonable assumptions, the exclusion of these costs serves to downwardly bias our main result – that subsidizing the procurement of peas and carrots increases profits by more than 60%. We formalize this argument through a simple model.

Suppose that a vendors' baseline profits, without scaling their business to include additional peas and carrots, generates r_0 revenue and c_0 expenses from vegetable procurement (i.e. the revenues and costs that we measure at baseline). And suppose that conditional on scaling their business to include additional peas and carrots it would generate $r_1 = sr_0$ revenue and $c_1 = sc_0$ expenses from vegetable procurement, for some scalar $s > 0$. Then our treatment effect corresponds to

$$\frac{(r_1 - c_1) - (r_0 - c_0)}{r_0 - c_0} = s \equiv \hat{\tau}.$$

Now consider any unmeasured fixed cost f – i.e. costs that do not scale with vegetable procurement – such as the rent expenses of a vendor's market stall.[12] Accounting for these fixed costs f would serve to increase our estimate of the impact of scaling the vendor's business on her profits to

$$\frac{(r_1 - c_1) - (r_0 - c_0)}{r_0 - c_0 - f} > \hat{\tau}.$$

Having established that properly accounting for fixed costs would serve to increase our estimated treatment effect, we now assume them to be zero and turn to unmeasured variable costs v, which scale with the amount of produce procured. These would include the cost of transporting the additional produce as well as the opportunity cost of the vendor's labor required to procure and sell the additional produce. We begin by assuming that the total unmeasured variable cost scales proportionately with the costs of vegetable procurement – the variable cost is vc_0 at the baseline scale, and is vc_1 at the larger scale induced by our intervention. Then accounting for these variable costs would not change the estimate of our impact. That is,

[12] In our survey of 391 vendors four years after the experiment concluded, the mean reported monthly cost of selling in the market is INR 6,459, or roughly 80 USD.

$$\frac{(r_1-(1+v)c_1)-(r_0-(1+v)c_0)}{r_0-(1+v)c_0}=\hat{\tau}$$

Departing from our assumption that unmeasured variable costs scale proportionately with the costs of vegetable procurement, if instead the unmeasured variable costs scaled less than proportionately, accounting for the variable costs would only serve to increase our estimated treatment effect. Therefore, the only unaccounted for expenses that could weaken our results are variable expenses that scale *more* than proportionately with the cost of vegetable procurement. We do not believe these types of variable expenses are likely. For instance, in our 2023 survey of 391 vegetable vendors, the mean cost of transportation for one round of procurement is INR 198. When asked the cost of transportation if the procurement quantity was doubled, the average vendor reports a 67% increase in cost, and 97% of vendors report an increase of 100% or less. This suggests that transport costs scale less than proportionately with the cost of procurement, consistent with quantity discounts.

Regarding labor costs, we note that in our setting, hired labor is extremely rare. We asked vendors how they would manage any extra work if they were to expand their business. Only 7% said that they would hire workers. Relatedly, no vendor reports that they would arrange for childcare. This leaves the (unmeasured) opportunity cost of the vendor's labor. In response to the same question, 48% of vendors say that they would arrive at the retail market earlier or stay later, while 30% say that they would work harder while at the market. However from above, for our estimate of the proportional treatment effect on profits to be an overestimate, it needs to be that the opportunity cost of the vendor's time went up by more than 60% in the subsidy period. Given that they already work 50 hours a week, and that is close to the entire time the market is open, only a small part of this 60% increment can from more hours. Which leaves a very large (>60%) increase in the psychic cost of the average hour worked as the only possible source of upward bias in our estimate of the proportional change in profits. While we do not have a way to formally reject that possibility it seems highly unlikely. As pointed out by Agness et al. (2022), the literature in fact often assumes that the opportunity cost is zero, and this reflects a sense that the self-employed do not act as if the psychic cost of their time is important for them.[13] It seems implausible therefore that this cost would suddenly go up very sharply.

6 Alternative Explanations

Thus far we have established the presence of anti-competitive norms and argued that, by deviating from these norms, vendors could increase their profits by more than 60% *at the market level*. In this section we rule out several alternative explanations of our results.

Are vendors in fact maximizing their expected utility from profits, given their constraints? To rule out this possibility, we must demonstrate that selling more peas and carrots is profitable, entails little risk, and is feasible without outside intervention.

The evidence that it is profitable has already been presented. However, in principle, a sufficiently high degree of risk or loss aversion can explain failures to adopt profitable business practices (e.g. Kremer et al., 2013). But in fact stocking peas and carrots offered a high return with little risk. In treatment markets during the subsidy period, vendors who sold carrots earned positive profits from doing so on 96.1% of the vendor $\times$ days in which they stocked carrots, and 99.6% of the vendor $\times$ weeks. 100% of vendors earned positive profits over the full subsidy period. The analogous numbers for peas are 96.2%, 99.1%, and 98.9%.

Given these statistics, even an extremely risk-averse vendor ought to find it worthwhile to stock at least a small amount of peas and carrots. Yet we found that the probability a vendor stocked any carrots or peas fell to just 10% and 5% respectively after the subsidy removal.

Finally, we rule out that external constraints bind. Our experimental design ensures that vendors who procured additional peas and carrots during the subsidy period had access to all of

[13] Agness et al. (2022) suggests that this reflects behavioral biases and a more welfare relevant valuation of their time should be 60% of the market wage. But, of course, in understanding the behavior of the vendors what matters is their perceived value of their time.

the necessary capital, labor, and skill required to do so. Specifically, each day the subsidy was delivered to vendors only after they procured the additional vegetables on their own. Therefore a lack of any of these factors cannot be the explanation for why vendors did not stock peas and carrots before our intervention, or continue to do so after it concluded.

Do vendors know that selling (more) peas and carrots would increase their profits? We note that even if vendors were not aware that selling additional produce would be profitable, this would not pose a threat to our interpretation that abiding by the market norms suppresses profits. Nevertheless, our experiment strongly suggests that a lack of knowledge about the profitability of selling peas and carrots is not the inhibiting factor. Vendors in our treatment markets experienced higher profits from selling peas and carrots for three weeks, and despite this, they largely ceased selling the additional products once the subsidy was removed.

While in principle it is possible that vendors did not realize they were earning higher profits than in their unsubsidized counterfactual, we do not believe this is likely. This is a setting in which learning the profitability of selling a new product requires neither complicated accounting nor inference about a counterfactual scenario. So long as the revenues generated by selling peas and carrots exceed the cost of procuring them – a fact that is clearly satisfied in our setting – and so long as vendors have sufficient excess capacity to stock additional produce without removing any of their existing produce, then selling the additional products should result in increased profits. This latter fact is confirmed in Table 2, demonstrating that the sales of peas and carrots complemented, rather than displaced sales of existing produce.

For these reasons, it is not likely that vendors ceased selling peas and carrots for lack of knowledge that doing so would be profitable.

7 Discussion and Conclusion

We present descriptive evidence of competition-suppressing norms in Kolkata vegetable markets; vendors report likely negative consequences if they were to lower their prices by 10%, if they were to double the size of their business, or if they were to newly stock peas and carrots.

We further present evidence from a market-level experiment in which we induced a temporary relaxation of these norms by subsidizing vendors to stock additional peas and carrots. Profits increased by 60% at the market level. No external constraint prevented vendors from exploiting this opportunity. And even after we subsidized vendors to stock additional produce, allowing them to directly experience the degree to which doing so would increase their profits, almost all vendors reduced or stopped the sale of these produce altogether upon the subsidy's removal. Our results suggest that these collusive norms not only suppress competition, but also reduce aggregate profits.

One important caveat to this conclusion is that, while we demonstrate that prevailing norms discourage the adoption of profitable opportunities, it is still possible that the counterfactual without such a norm would lead vendors to earn even lower profits. Simple and rigid rules of the kind that everyone sells only their designated goods could be necessary to maintain the status-quo level of collusion. For instance, perhaps it is easier to enforce norms of the type, "don't sell peas if too many of your neighbors are already doing so," than it is to enforce norms of the type, "don't add peas to your stall unless doing so would not hurt your neighbors." In the latter case, vendors might begin to sell new types of vegetables even when doing so does reduce their neighbors' profits so long as there is some plausible deniability. Though potentially easier to enforce, the cost of the simpler norms is that they may make it impossible for vendors to take advantage of new opportunities.

Our results have important implications for development research and policy. Development economists have long noted the ubiquity of small firms operating side-by-side in densely packed urban markets. Lewis (1954) observes that petty retail trading in developing countries is dominated by crowded markets and traders making only a few sales each. According to Lewis, consumers would be no worse off if many of these traders left the market, leaving others to expand. Our results indicate that collusive norms may be in part responsible for this phenomenon, and that relaxing these norms might not only increase economic efficiency but also vendors' profits (at least in partial equilibrium).

More generally, our evidence on the possibility of a sustained price premium, and limited competition in a market with many sellers, is important in how we think about policy attempts to benefit small sellers and consumers in developing country markets. The ease of colluding may explain why interventions aimed at making markets more competitive have had limited success so far (Mitra et al., 2018; Banerjee et al., 2019; Busso and Galiani, 2019).

One important question is then whether it is possible to realign deeply seated norms that are shown to suppress profits and efficiency. Relatedly, while in our setting temporary violations of the norm did not lead to a new norm, this may not always be true. We leave these as topics of future research.

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A Descriptive Survey Details

The table below summarizes the variables used from our 2023 descriptive survey of 391 vendors. The table is followed by descriptions of the survey questions underlying each variable.

	N	Mean	SD	Min	Median	Max
Male	391	.78	.41	0	1	1
Age of vendor (in years)	391	51	12.67	19	52	82
Years selling in this market	391	25	14.46	.25	25	60
Typical daily revenue (INR)	391	3,624	3248.16	100	3,000	40,000
Typical daily profit (INR)	391	578	445.41	25	500	5,000
Typical weekly hours	391	50	18.42	0	49	119
Typical minutes waiting per hour	391	24	8.25	1	20	60
Monthly market cost (INR)	391	6,459	6856.8	0	5,000	91,245
Transport cost per trip (INR)	391	198	535.29	0	150	10,000
Transport cost for double procurement (INR)	391	314	800.2	0	200	15,000
To expand: arrive earlier	391	.36	.48	0	0	1
To expand: stay later	391	.38	.49	0	0	1
To expand: extra visits	391	.013	.11	0	0	1
To expand: work harder per hour	391	.3	.46	0	0	1
To expand: arrange for childcare	391	0	0	0	0	0
To expand: hire people	391	.069	.25	0	0	1
To expand: other	391	.23	.42	0	0	1
How easy to work extra hour	391	1.8	1	0	2	3
How easy to work two extra hours	391	2.1	1.03	0	2	3
Why difficult: old age	311	.35	.48	0	0	1
Why difficult: health problem	311	.24	.43	0	0	1
Why difficult: not enough energy	311	.52	.5	0	1	1
Why difficult: household duties	311	.52	.5	0	1	1
Why difficult: other	311	.11	.31	0	0	1
Hopes: status quo	391	.85	.36	0	1	1
Hopes: want to expand	391	.34	.47	0	0	1
Hopes: want to contract	391	.12	.32	0	0	1
Hopes: want to move	391	.018	.13	0	0	1
Hopes: want to hire	391	.049	.22	0	0	1
Hopes: want new job	391	.013	.11	0	0	1
Hopes: want to retire	391	.023	.15	0	0	1
Hopes: other	391	.013	.11	0	0	1
New seller: acceptable?	391	1.8	.57	0	2	3
New seller: negative consequences?	391	1.2	.8	0	1	3
New seller reaction: become angry	110	.6	.49	0	1	1

	N	Mean	SD	Min	Median	Max
New seller reaction: spread info	110	.45	.5	0	0	1
New seller reaction: prevent from working	110	.46	.5	0	0	1
New seller reaction: steer customers away	110	.25	.43	0	0	1
New seller reaction: stop talking	110	.1	.3	0	0	1
New seller reaction: stop drinking/cards/visit	110	.036	.19	0	0	1
New seller reaction: stop lending	110	.064	.25	0	0	1
New seller reaction: other	110	.0091	.1	0	0	1
Vendor expand: acceptable?	391	1.9	.46	0	2	3
Vendor expand: negative consequences?	391	1.4	.84	0	1	3
Vendor undercut: acceptable?	391	1.7	.66	0	2	3
Vendor undercut: negative consequences?	391	1.3	.91	0	1	3
Vendor same price: acceptable?	391	1.9	.48	0	2	3
Kinship tax: prefer private money?	391	.046	.21	0	0	1
Agree with: want to expand	391	.48	.5	0	0	1
Prefer wage employment?	391	.16	.36	0	0	1
Prefer wage employment, same pay?	391	.16	.37	0	0	1

- Years selling in the market derives from the survey question “How long have you been selling in this market?”
- For daily revenue and profit, we ask “What is your typical daily revenue? (i.e. how much total money do you receive from your customers on a typical day)” and “What is your typical daily profit? (i.e. your typical daily revenue less your typical daily costs)”
- For typical weekly hours, we first ask vendors which days they typically work on, then we ask them how many hours they typically work on each of these days. We sum up these numbers across the seven days of the week.
- For typical minutes waiting, we ask “For how many minutes of your typical working hour do you spend waiting for customers (e.g. sitting idly, chatting with friends nearby, etc.)?”
- For monthly market cost, we ask “What do you pay to be able to sell in this market? (e.g. rent, payments to market association, to police, etc.)”

- For transport cost per trip, we ask “What is the typical procurement cost of the vegetables you would buy in one trip to the wholesale market?” followed by “How much does it cost to transport this amount of vegetables to the market?” Transport cost per trip is the answer to the latter question.

- For transport cost for double procurement, we ask “If you wanted to buy double the vegetables that you usually buy, how much would the transportation cost then?”

- For the variables beginning “To expand:”, we ask “If you wanted to expand your business (by selling 50% more produce), how would you manage the extra work?” The variables in the table are indicator variables for each of the possible answer options: (1) I would arrive at the retail market earlier, (2) I would stay at the retail market later, (3) I would do extra visits to sell at the retail market (e.g. an extra day at the weekend, or an extra evening during the week), (4) I would have to work harder for each hour that I am at the market, to make sure I sell the extra produce, (5) I would arrange for childcare so that I can work when I would usually be looking after my children, (6) I would hire people to work for me, and (7) other.

- For how easy to work extra hour, we ask “How easy would it be for you to work for 1 additional hour each day, if you wanted to? For example, suppose that customers were still shopping at the end of your work day, so that you could still earn your average hourly profit in that extra hour.” The answer options are 0 = very easy, 1 = somewhat easy, 2 = somewhat difficult, and 3 = very difficult.

- For how easy to work two extra hours, we ask “How easy would it be for you to work for 2 additional hours each day, if you wanted to? For example, suppose that customers were still shopping at the end of your work day, so that you could still earn your average hourly profit in the two extra hours.” The answer options are 0 = very easy, 1 = somewhat easy, 2 = somewhat difficult, and 3 = very difficult.

- For those that respond that it would be somewhat or very difficult to work one or two extra hours, we ask "Why would it be difficult?" The answer options are (1) old age, (2) health problem, (3) not enough energy to work additional hours, (4) need to tend to household duties, and (5) other. Indicator variables for each of these answers are included in the table with names "Why difficult: ..."

- "Hopes:" variables derive from the question "What are your main hopes for your business over the coming year?" We did not read out answer categories to the vendors for this question. Surveyors selected from the following categories: (1) no hopes/status quo, (2) want to expand/sell more, (3) want to contract/sell less, (4) want to move to a different market, (5) want to hire people to work for me, (6) want to stop selling vegetables and shift into another job, (7) want to retire from working, and (8) other.

- For new seller: acceptable?, we asked "Suppose a vendor in this market who had never sold carrots or peas before began to sell carrots or peas. Do you think this behavior is acceptable?" The answer categories are: 0 = highly unacceptable, 1 = unacceptable, 2 = acceptable, and 3 = highly acceptable.

- For new seller: negative consequences?, we asked "Suppose a vendor in this market who had never sold carrots or peas before began to sell carrots or peas. How likely is it that he would face negative conesquences from other vendors in this market?" The answer categories are: 0 = highly unlikely, 1 = somewhat unlikely, 2 = somewhat likely, and 3 = highly likely.

- We then ask "Suppose a vendor in this market who had never sold carrots or peas before began to sell carrots or peas. What reactions will he face from the other vendors in the market?" 110 vendors reported at least one reaction. Among these vendors, we include indicator variables in the table above for each type of reaction reported: (1) become angry, (2) spread information to other vendors/markets, (3) prevent from working at market,

(4) steer customers away, (5) social sanctions – stop talking, (6) social sanctions – stop drinking/playing cards/visiting home, (7) stop lending money, and (8) other.

- For vendor expand: acceptable?, we asked "Suppose a vendor in this market worked hard to expand their business, stocking more produce, and almost doubling their business in size over a few months. Do you think that this behavior is acceptable?" The answer categories are: 0 = highly unacceptable, 1 = unacceptable, 2 = acceptable, and 3 = highly acceptable.

- For vendor expand: negative consequences?, we asked "Suppose a vendor in this market worked hard to expand their business, stocking more produce, and almost doubling their business in size over a few months. How likely is it that he would face negative consequences from other vendors in this market?" The answer categories are: 0 = highly unlikely, 1 = somewhat unlikely, 2 = somewhat likely, and 3 = highly likely.

- For vendor undercut, acceptable?, we asked "Suppose a vendor in this market were to sell similar produce to yours but at 10% lower price. Do you think this behavior is acceptable?" The answer categories are: 0 = highly unacceptable, 1 = unacceptable, 2 = acceptable, and 3 = highly acceptable.

- For vendor undercut, negative consequences?, we asked "Suppose a vendor in this market were to sell similar produce to yours but at 10% lower price. How likely is it that he would face negative consequences from other vendors in this market?" The answer categories are: 0 = highly unlikely, 1 = somewhat unlikely, 2 = somewhat likely, and 3 = highly likely.

- For vendor same price: acceptable?, we asked "Suppose a vendor in this market were to sell similar produce to yours at a similar price. Do you think this behavior is acceptable?" The answer categories are: 0 = highly unacceptable, 1 = unacceptable, 2 = acceptable, and 3 = highly acceptable.

- For kinship tax: prefer private money?, we asked “Imagine that I offer you INR 500 today. Now what if I gave you the choice of not telling your extended family and friends that I gave you money. Then they would not know that you received any money from me. If you could choose either, (1) I give you INR 500 and I announce this to your extended family and friends, or (2) I give you INR 400 and do not tell your extended family and friends, which would you prefer?” The variable in the table is an indicator variable equal to one if the vendor selected the second option.

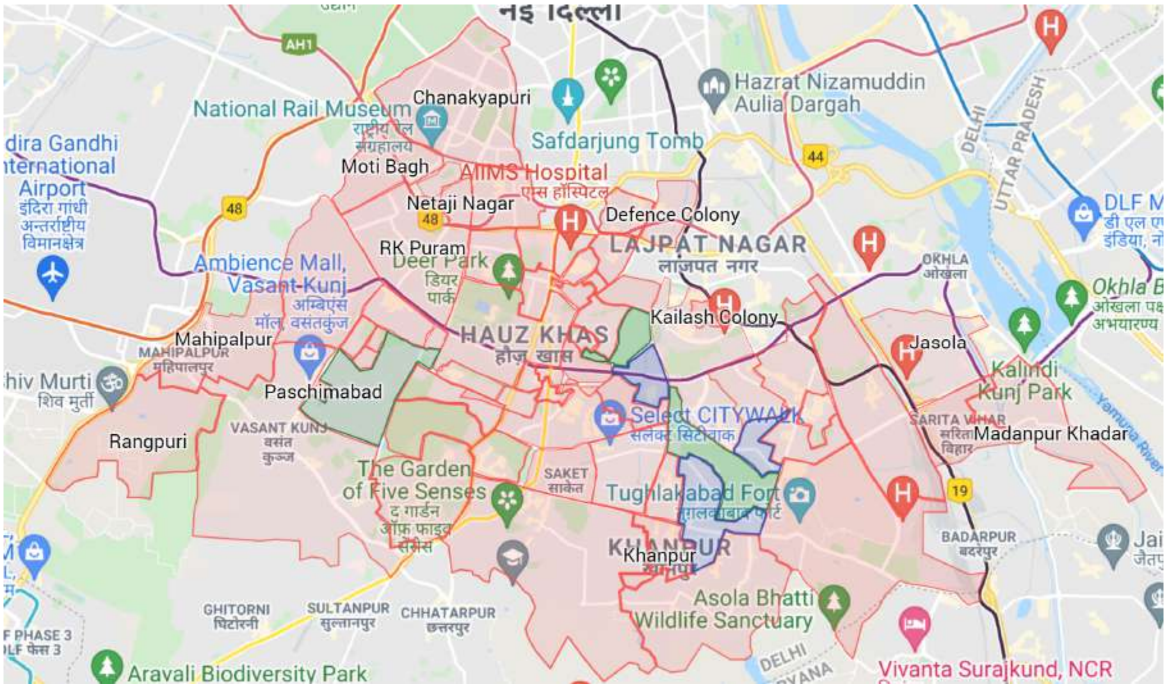
- For agree with: want to expand, we asked “I’m now going to read out a list of options for your hopes for your business. We want to know if you agree or disagree with each option as one of your hopes for the business.” One of the options was “want to expand/sell more.” The indicator variable in the table is equal to one if the vendor agreed with this option.

- For prefer wage employment?, we asked “Suppose someone offered you wage employment at the prevailing wage, for a task that you are capable of doing. Would you prefer this job to your current job selling vegetables?”

- For prefer wage employment, same pay?, we asked “How about if the job was not at the prevailing wage, but paying the same amount that you earn selling vegetables. Would you prefer this job to selling vegetables?”

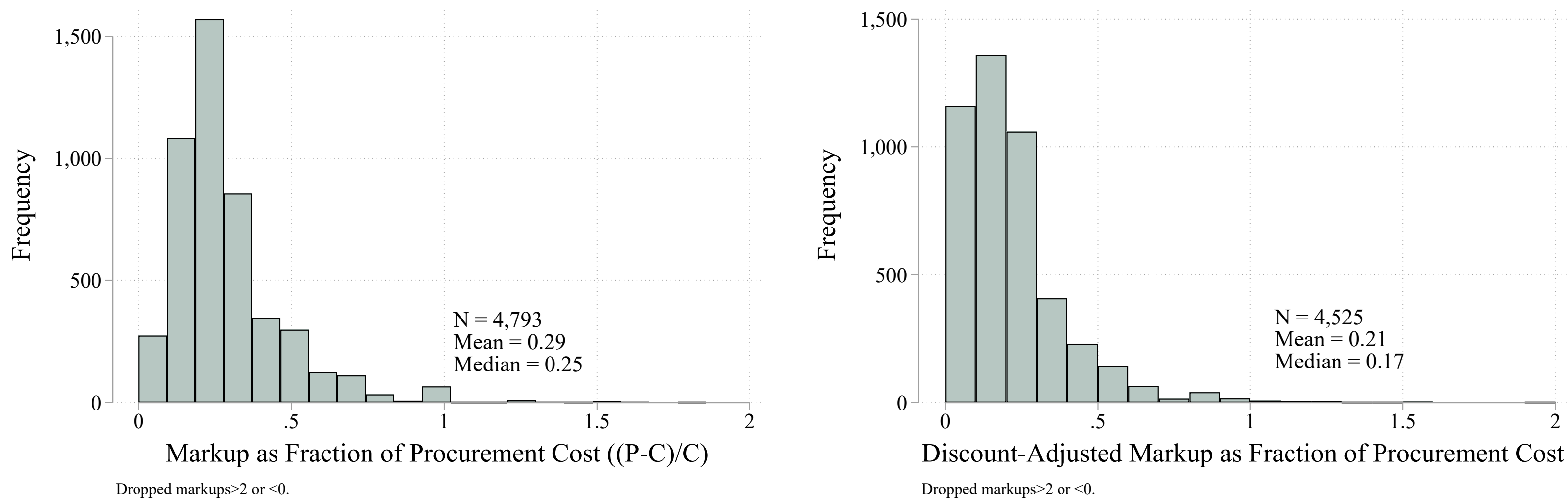
B Appendix Figures and Tables

Figure A1: Fruit Vendor Census Area



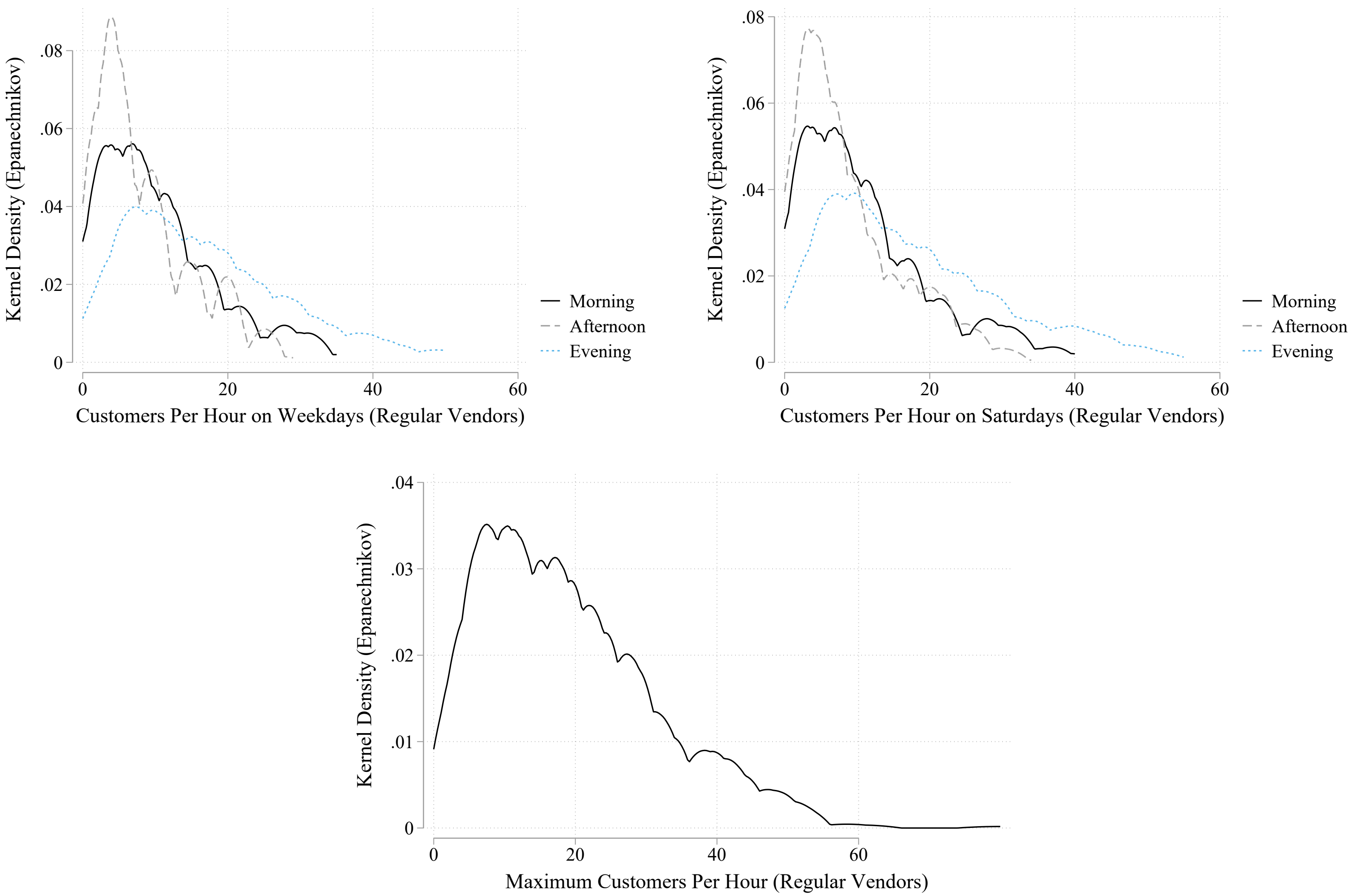
Notes: This figure shows the contiguous 135 square kilometer area of South Delhi covered by our vendor census. The red polygons cover 125 square kilometers and were successfully surveyed, the green polygons (Jawaharlal Nehru University, Hauz Khas Forest, and Jahanpanah Forest) are non-commercial areas and so were not surveyed, while the three blue polygons were erroneously missed.

Figure A2: The Distribution of Fruit-Level Markups



Notes: The figure shows the distribution of fruit-level markups as measured in the vendor census survey. In the left panel, the markup is measured as the stated selling price less the stated procurement cost, as a fraction of the stated procurement cost. In the right panel, we discount-adjust each markup by subtracting the vendor's stated typical discount (with the discount only measured at the vendor-level, rather than fruit-by-fruit). In both cases, we drop outliers above two or below zero (25 dropped from the left panel, 246 dropped from the right).

Figure A3: Many Vendors Are Not Very Busy



Notes: The top panel shows kernel densities of regular vendor answers to the question "what is the number of customers you serve during one hour of operations during each of these time periods on a [weekday/Saturday]?" In each case, the figure excludes any outlier answers at or above the 99th percentile. The bottom panel shows the kernel density of the maximum answer given to the six previous questions (weekday/Saturday-by-morning/afternoon/evening), as well as the equivalent questions for Sunday, which were only asked in the rare case that vendors said Sunday demand was different to Saturday demand.

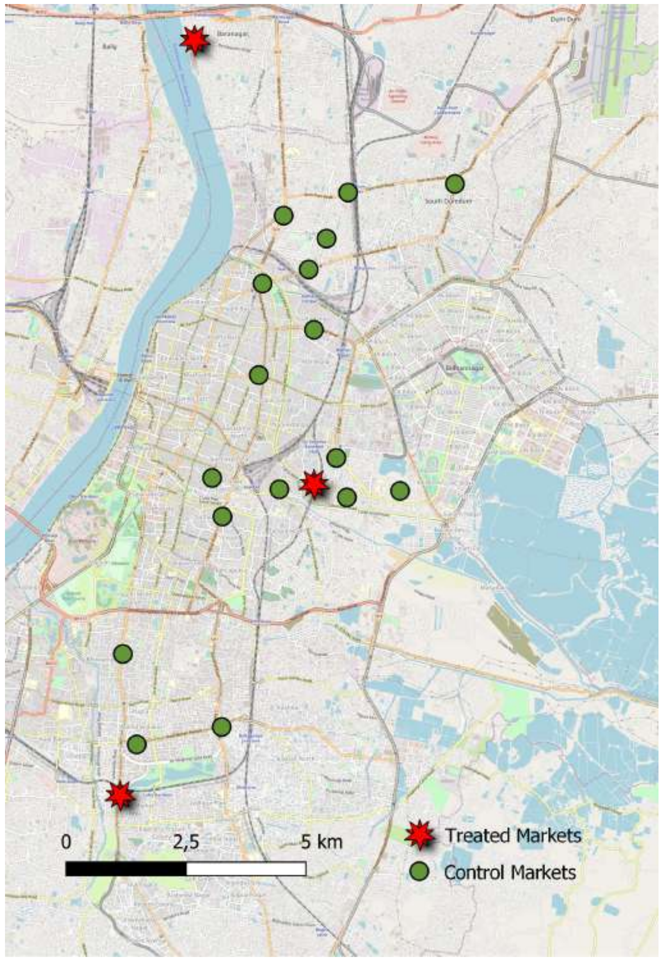
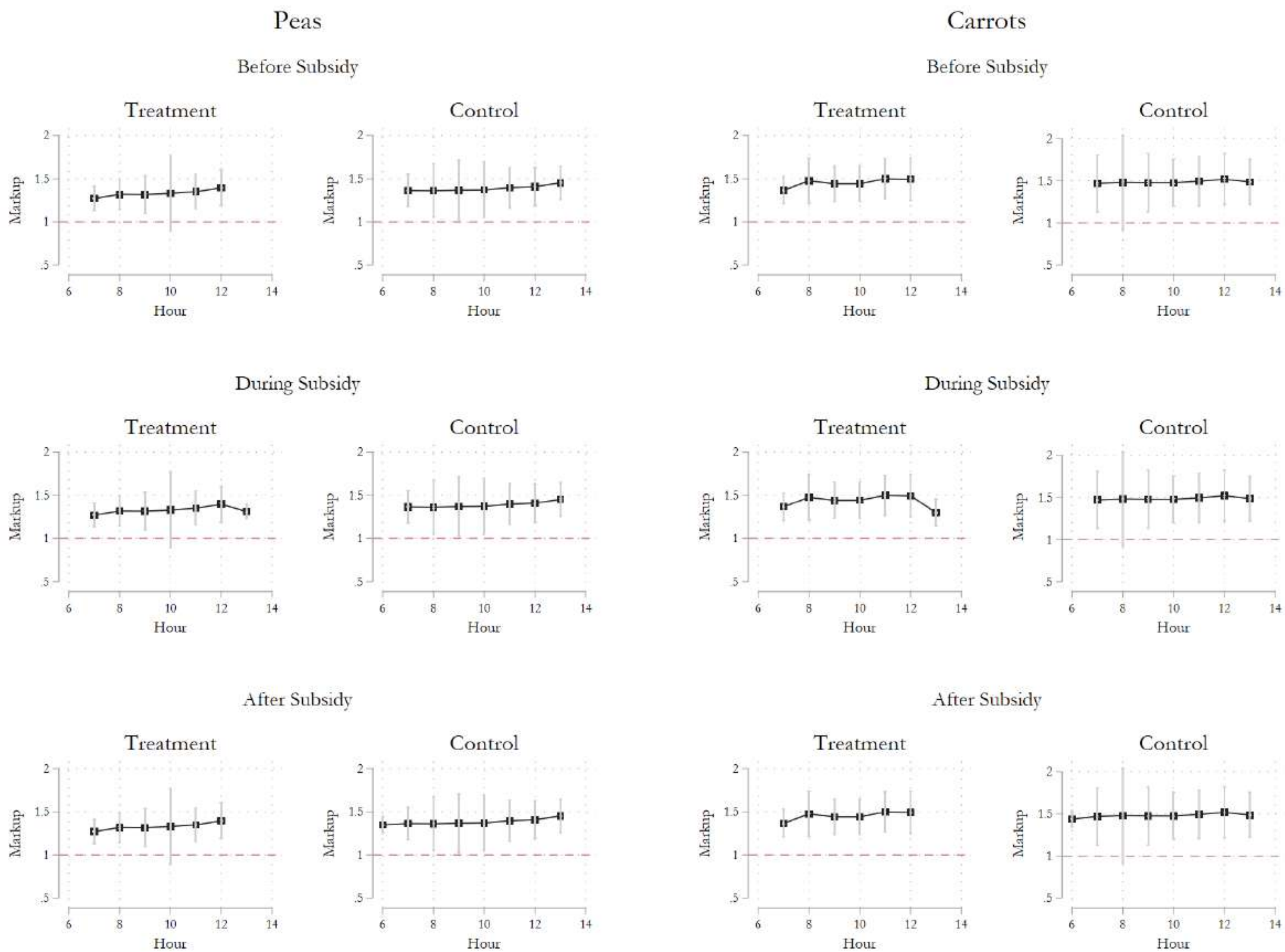


Figure A4: This map shows the location of the treated markets (in red) and control markets (in green) in our Kolkata sample.

Figure A5: Vegetable Markup by Hour



Notes: The figure illustrates the markup of Peas and Carrots during different survey hours. The markup is calculated as the ratio of retail price to wholesale market cost. The first row displays the markup evolution in the pre-subsidy period, the second row shows the subsidy period, and the third row represents the post-period. Each period is further divided into different plots for both the treatment and control groups.

Table A1: Descriptive Statistics for Intervention and Control Markets

	Charu Market (1)	Sarkar Bazar (2)	Alam Bazar (3)	Control markets (4)
Mean # vendors in census	45.0	73.0	85.0	85.8
Mean # vendors present per day	34.5	57.4	71.7	86.7
Mean profits per vendor (Rs.)	448.9	344.9	314.6	519.6
Mean # vegetables available per vendor	5.7	5.1	4.0	5.2
% of present vendors selling peas	62.9	59.9	51.8	61.0
% of present vendors selling carrots	65.1	52.2	39.1	54.1
Mean total cost of daily purchases (Rs.)	969.0	894.4	748.3	1,396.8
Mean value of Sales (Rs.)	1,417.9	1,239.4	1,062.9	1,917.0
Mean number of years selling in market	22.0	22.9	26.6	23.9
Mean age	44.2	48.7	49.1	47.6
% of female vendors	33.3	47.9	12.9	23.3

Notes: This table presents summary statistics averaged for each intervention market in columns 1 - 3 and averaged over all control markets in column 4. All statistics are calculated using data from the pre-subsidy period. Each of the control markets is assigned equal weight in the reported mean. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

Table A2: Testing for Parallel Trends in the Pre-Subsidy Period

	Carrot				Peas			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
Panel A: Carrot and Peas								
β_3 Treat	–0.05	–3.21	–1.51	–13.06	–0.08	–0.44	–3.92	–32.10
	[–0.248, 0.193]	[–7.777, 0.852]	[–3.496, 0.580]	[–28.925, 7.708]	[–0.360, 0.220]	[–5.608, 4.298]	[–8.219, -0.212]	[–68.302, 1.433]
	{ 0.700 }	{ 0.085 }	{ 0.135 }	{ 0.294 }	{ 0.543 }	{ 0.753 }	{ 0.045 }	{ 0.056 }
	⟨ 0.606 ⟩	⟨ 0.181 ⟩	⟨ 0.218 ⟩	⟨ 0.182 ⟩	⟨ 0.504 ⟩	⟨ 0.789 ⟩	⟨ 0.013 ⟩	⟨ 0.021 ⟩
γ_1 Treat × Day	–0.00	0.08	0.04	0.17	0.01	0.06	0.19	–0.22
	[–0.014, 0.009]	[–0.313, 0.532]	[–0.071, 0.140]	[–1.575, 2.185]	[–0.005, 0.016]	[–0.296, 0.414]	[–0.131, 0.540]	[–3.323, 3.374]
	{ 0.981 }	{ 0.696 }	{ 0.433 }	{ 0.718 }	{ 0.594 }	{ 0.701 }	{ 0.628 }	{ 0.747 }
	⟨ 0.991 ⟩	⟨ 0.626 ⟩	⟨ 0.529 ⟩	⟨ 0.747 ⟩	⟨ 0.446 ⟩	⟨ 0.605 ⟩	⟨ 0.517 ⟩	⟨ 0.839 ⟩
Pre-subsidy intervention market mean	0.488	27.881	3.398	24.946	0.566	41.749	5.715	44.499
Number of Vendors	1591	1361	1591	1591	1591	1373	1591	1591
Number of Observations	20040	10675	20040	20040	20040	12053	20040	20040

	Cost of wholesale purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
Panel B: Aggregate				
β_3 Treat	–550.19	–689.20	–139.01	–0.40
	[–1189.504, 3.171]	[–1201.552, -52.897]	[–305.242, 50.550]	[–2.780, 1.980]
	{ 0.051 }	{ 0.046 }	{ 0.204 }	{ 0.631 }
	⟨ 0.069 ⟩	⟨ 0.052 ⟩	⟨ 0.154 ⟩	⟨ 0.644 ⟩
γ_1 Treat × Day	13.02	12.88	–0.14	–0.01
	[–8.966, 33.517]	[–14.810, 40.492]	[–9.291, 10.210]	[–0.091, 0.076]
	{ 0.505 }	{ 0.632 }	{ 0.980 }	{ 0.334 }
	⟨ 0.367 ⟩	⟨ 0.517 ⟩	⟨ 0.970 ⟩	⟨ 0.455 ⟩
Pre-subsidy intervention market mean	825.121	1167.304	342.183	4.733
Number of Vendors	1591	1591	1591	1591
Number of Observations	20040	20040	20040	20040

Notes: This table estimates the following specification: $y_{it} = \alpha + \beta_1 Day_t + \beta_2 Treat_m + \beta_3 Day_t \times Treat_m + \varepsilon_{imt}$ on our sample during the pre-subsidy period. Coefficients for Day not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. In Panel A, outcomes are specific to peas or carrots. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention. In Panel B the outcomes correspond to aggregate measures. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day.

Table A3: Subsidy Impacts on Carrots: By Pre-Period Carrot Sales

	Eligible				Ineligible			
	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_3 Treat	–0.03	0.14	–0.88	–7.44	–0.01	–3.06	–1.32	–13.12
	[–0.101, 0.040]	[–8.571, 11.088]	[–2.271, 0.142]	[–20.559, 3.236]	[–0.151, 0.140]	[–7.143, 2.125]	[–5.089, 2.331]	[–41.382, 16.092]
	{ 0.203 }	{ 0.939 }	{ 0.057 }	{ 0.072 }	{ 0.934 }	{ 0.090 }	{ 0.167 }	{ 0.270 }
	⟨ 0.439 ⟩	⟨ 0.947 ⟩	⟨ 0.006 ⟩	⟨ 0.039 ⟩	⟨ 0.928 ⟩	⟨ 0.086 ⟩	⟨ 0.267 ⟩	⟨ 0.211 ⟩
γ_1 Treat × During Subs	0.70	–1.87	5.85	44.58	0.45	0.16	6.00	44.78
	[0.582, 0.751]	[–5.377, 2.017]	[4.757, 6.764]	[29.547, 54.127]	[0.229, 0.662]	[–3.024, 3.344]	[3.750, 8.648]	[17.312, 69.357]
	{ < 0.001 }	{ 0.554 }	{ < 0.001 }	{ 0.002 }	{ 0.017 }	{ 0.816 }	{ 0.003 }	{ 0.026 }
	⟨ < 0.001 ⟩	⟨ 0.361 ⟩	⟨ < 0.001 ⟩	⟨ 0.003 ⟩	⟨ < 0.001 ⟩	⟨ 0.865 ⟩	⟨ < 0.001 ⟩	⟨ 0.002 ⟩
γ_2 Treat × After Subs	0.12	–1.59	1.85	11.21	0.09	–2.11	1.86	6.94
	[0.045, 0.176]	[–8.995, 4.909]	[0.615, 2.576]	[2.744, 18.805]	[–0.157, 0.340]	[–9.917, 6.610]	[–1.607, 5.068]	[–34.375, 43.041]
	{ 0.019 }	{ 0.275 }	{ 0.018 }	{ 0.032 }	{ 0.399 }	{ 0.205 }	{ 0.101 }	{ 0.379 }
	⟨ 0.049 ⟩	⟨ 0.466 ⟩	⟨ 0.012 ⟩	⟨ 0.004 ⟩	⟨ 0.482 ⟩	⟨ 0.240 ⟩	⟨ 0.312 ⟩	⟨ 0.482 ⟩
Pre-subsidy intervention market mean	0.151	27.917	0.844	6.739	0.785	27.913	5.687	41.665
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.925	<0.001	<0.001	0.006	0.164	0.019	0.040
Fisher p-value: $\gamma_1 = \gamma_2$	<0.001	0.941	<0.001	0.002	0.002	0.165	<0.001	<0.001
Number of Vendors	629	517	629	629	1002	953	1002	1002
Number of Observations	22045	4408	22045	22044	33173	20665	33173	33169

Notes: This table estimates specification 1 on our full sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for vendors who sold carrots on less than 8 days during the pre-subsidy period (analogous to the pea subsidy eligibility criterion) and 5 - 8 present outcomes for vendors who sold carrots on 8 or more days during the pre-subsidy period. The outcome in columns 1 and 5 is whether the vendor sells carrots on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for carrots, the outcome in columns 3 and 7 measure the wholesale quantity of carrots procured, and the outcome in columns 4 and 8 measure the daily profits accrued from carrots. Profits are calculated by computing (amount of carrots at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Table A4: Subsidy Impacts: Carrots and Peas, Removing Control Markets Most Likely to be Impacted by Demand Spillovers

	Carrot				Peas			
	Prob. of selling (%) (1)	Sale price (Rs.) (2)	Wholesale qty. bought (kg) (3)	Profits (Rs.) (4)	Prob. of selling (%) (5)	Sale price (Rs.) (6)	Wholesale qty. bought (kg) (7)	Profits (Rs.) (8)
β_3 Treat	–0.04	–2.41	–1.29	–10.66	–0.04	0.40	–2.53	–30.34
	[–0.242, 0.195]	[–6.057, 1.717]	[–3.354, 1.312]	[–23.827, 9.896]	[–0.338, 0.215]	[–2.229, 2.834]	[–7.383, 1.816]	[–66.253, 14.929]
	{ 0.685 }	{ 0.088 }	{ 0.295 }	{ 0.311 }	{ 0.542 }	{ 0.614 }	{ 0.088 }	{ 0.072 }
	⟨ 0.666 ⟩	⟨ 0.131 ⟩	⟨ 0.312 ⟩	⟨ 0.168 ⟩	⟨ 0.618 ⟩	⟨ 0.654 ⟩	⟨ 0.150 ⟩	⟨ 0.066 ⟩
γ_1 Treat × During Subs	0.58	0.00	5.99	44.95	0.40	–0.59	6.84	59.02
	[0.397, 0.751]	[–2.287, 2.445]	[3.717, 7.707]	[21.165, 57.910]	[0.204, 0.641]	[–3.864, 3.092]	[4.073, 11.220]	[16.323, 100.547]
	{ 0.002 }	{ 1.000 }	{ < 0.001 }	{ 0.001 }	{ 0.013 }	{ 0.810 }	{ 0.012 }	{ 0.032 }
	⟨ 0.001 ⟩	⟨ 1.000 ⟩	⟨ 0.001 ⟩	⟨ 0.001 ⟩	⟨ < 0.001 ⟩	⟨ 0.775 ⟩	⟨ 0.001 ⟩	⟨ 0.001 ⟩
γ_2 Treat × After Subs	0.11	–1.08	2.01	11.29	0.06	–0.66	2.58	25.22
	[–0.063, 0.262]	[–3.624, 1.669]	[0.004, 4.060]	[–3.038, 24.198]	[–0.226, 0.316]	[–4.191, 2.214]	[–1.707, 7.546]	[–32.502, 75.147]
	{ 0.339 }	{ 0.309 }	{ 0.048 }	{ 0.173 }	{ 0.433 }	{ 0.662 }	{ 0.076 }	{ 0.091 }
	⟨ 0.297 ⟩	⟨ 0.312 ⟩	⟨ 0.176 ⟩	⟨ 0.100 ⟩	⟨ 0.504 ⟩	⟨ 0.600 ⟩	⟨ 0.093 ⟩	⟨ 0.075 ⟩
Pre-subsidy intervention market mean	0.490	27.914	3.433	25.407	0.569	41.798	5.939	47.727
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.425	<0.001	<0.001	0.002	0.972	0.002	0.013
Fisher p-value: $\gamma_1 = \gamma_2$	0.001	0.460	0.001	<0.001	0.001	0.974	<0.001	0.007
Number of Vendors	1477	1329	1477	1477	1477	1351	1477	1477
Number of Observations	50042	22173	50042	50037	50060	20334	50060	50058

Notes: This table replicates Table 1 excluding control markets that were frequently cited as a likely substitute for each treatment market. Substitute control markets were defined as any of top three responses by vendors in treatment markets to this question: 'If customers were not buying from this market, where would they buy?' Relative to our full sample, this sample excludes three control markets, as the majority of responses to this question were markets that are not in our sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for carrots, and 5 - 8 for peas. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Table A5: Subsidy Impacts: Aggregate, Removing Control Markets Most Likely to be Impacted by Demand Spillovers

	Aggregate			
	Total Cost of Wholesale Purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
	(1)	(2)	(3)	(4)
β_3 Treat	–439.42	–563.24	–123.82	–0.46
	[–1063.251, 18.057]	[–1290.848, -49.077]	[–333.440, 154.260]	[–2.551, 1.973]
	{ 0.050 }	{ 0.047 }	{ 0.181 }	{ 0.570 }
	⟨ 0.049 ⟩	⟨ 0.047 ⟩	⟨ 0.107 ⟩	⟨ 0.491 ⟩
γ_1 Treat $\times$ During Subs	712.69	952.08	239.33	2.06
	[261.979, 1186.478]	[291.745, 1620.608]	[–68.883, 518.041]	[0.758, 3.195]
	{ 0.025 }	{ 0.027 }	{ 0.059 }	{ 0.017 }
	⟨ 0.001 ⟩	⟨ 0.004 ⟩	⟨ 0.021 ⟩	⟨ 0.012 ⟩
γ_2 Treat $\times$ After Subs	561.93	542.41	–19.52	1.26
	[–100.174, 1103.259]	[–216.245, 1166.595]	[–294.265, 208.509]	[–0.466, 2.744]
	{ 0.234 }	{ 0.291 }	{ 0.687 }	{ 0.280 }
	⟨ 0.221 ⟩	⟨ 0.304 ⟩	⟨ 0.776 ⟩	⟨ 0.200 ⟩
Pre-subsidy intervention market mean	1117.143	1556.492	439.340	5.493
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	0.559	0.252	0.013	0.037
Fisher p-value: $\gamma_1 = \gamma_2$	0.597	0.201	0.003	0.053
Number of Vendors	1474	1474	1474	1474
Number of Observations	48098	48098	48098	48098

This table replicates Table 2 excluding control markets that were frequently cited as a likely substitute for each treatment market. Substitute control markets were defined as any of top three responses by vendors in treatment markets to this question: 'If customers were not buying from this market, where would they buy?' Relative to our full sample, this sample excludes three control markets, as the majority of responses to this question were markets that are not in our sample. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.

Table A6: Subsidy Impacts: Carrots and Peas, Removing Control Vendors Most Likely to be Impacted by Supply Spillovers

	Carrot				Peas			
	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)	Prob. of selling (%)	Sale price (Rs.)	Wholesale qty. bought (kg)	Profits (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_3 Treat	0.034	–1.542	–0.590	–5.659	0.033	0.198	–1.332	–18.720
	[–0.127, 0.246]	[–4.198, 1.109]	[–2.079, 1.695]	[–14.560, 14.961]	[–0.220, 0.263]	[–2.147, 2.754]	[–4.491, 1.360]	[–49.959, 12.520]
	{ 0.612 }	{ 0.258 }	{ 0.553 }	{ 0.362 }	{ 0.627 }	{ 0.813 }	{ 0.392 }	{ 0.103 }
	⟨ 0.713 ⟩	⟨ 0.294 ⟩	⟨ 0.585 ⟩	⟨ 0.488 ⟩	⟨ 0.676 ⟩	⟨ 0.869 ⟩	⟨ 0.527 ⟩	⟨ 0.286 ⟩
γ_1 Treat × During Subs	0.560	0.294	5.594	43.377	0.387	0.551	6.125	52.330
	[0.394, 0.716]	[–1.529, 2.265]	[3.338, 7.370]	[19.111, 56.000]	[0.216, 0.586]	[–2.803, 3.978]	[4.199, 8.571]	[27.659, 72.489]
	{ < 0.001 }	{ 0.766 }	{ < 0.001 }	{ 0.001 }	{ 0.006 }	{ 0.754 }	{ 0.007 }	{ 0.010 }
	⟨ 0.006 ⟩	⟨ 0.801 ⟩	⟨ 0.010 ⟩	⟨ 0.012 ⟩	⟨ 0.016 ⟩	⟨ 0.833 ⟩	⟨ 0.005 ⟩	⟨ < 0.001 ⟩
γ_2 Treat × After Subs	0.110	–1.511	1.748	10.362	0.043	–0.822	1.893	18.474
	[–0.070, 0.258]	[–3.647, 0.446]	[–0.188, 3.831]	[–4.613, 20.413]	[–0.147, 0.259]	[–4.521, 1.357]	[–0.839, 4.553]	[–6.564, 39.340]
	{ 0.378 }	{ 0.168 }	{ 0.087 }	{ 0.191 }	{ 0.566 }	{ 0.614 }	{ 0.075 }	{ 0.061 }
	⟨ 0.396 ⟩	⟨ 0.214 ⟩	⟨ 0.218 ⟩	⟨ 0.162 ⟩	⟨ 0.726 ⟩	⟨ 0.604 ⟩	⟨ 0.275 ⟩	⟨ 0.182 ⟩
Pre-subsidy intervention market mean	0.490	27.914	3.433	25.407	0.569	41.798	5.939	47.727
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	<0.001	0.266	<0.001	<0.001	<0.001	0.545	0.001	<0.001
Fisher p-value: $\gamma_1 = \gamma_2$	0.004	0.344	<0.001	0.004	0.012	0.704	0.025	0.028
Number of Vendors	1013	888	1013	1013	1013	898	1013	1013
Number of Observations	33581	13556	33581	33576	33615	12386	33615	33613

Notes: This table replicates Table 1 excluding vendors that frequently buy in the two most popular wholesale markets among treated vendors in the sample, Kole Market and Sealdaha. Relative to our full sample, this sample excludes 716 vendors. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. Columns 1 - 4 present outcomes for carrots, and 5 - 8 for peas. The outcome in columns 1 and 5 is whether the vendor sells carrots or peas on the given day, the outcome in columns 2 and 6 measure the vendor's anticipated sale price for the relevant vegetable, the outcome in columns 3 and 7 measure the wholesale quantity procured of the relevant vegetable, and the outcome in columns 4 and 8 measure the daily profits accrued from the relevant vegetables. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured. Our measure of profit does not include the subsidy vendors received as part of our intervention.

Table A7: Subsidy Impacts: Aggregate, Removing Control Vendors Most Likely to be Impacted by Supply Spillovers

	Aggregate			
	Total Cost of Wholesale Purchases (Rs.)	Sales (Rs.)	Profits (Rs.)	# vegetables available
	(1)	(2)	(3)	(4)
β_3 Treat	–349.249	–459.819	–110.570	–0.126
	[–817.293, 51.319]	[–1061.436, 105.553]	[–304.892, 79.456]	[–1.944, 1.674]
	{ 0.063 }	{ 0.066 }	{ 0.176 }	{ 0.853 }
	⟨ 0.147 ⟩	⟨ 0.135 ⟩	⟨ 0.243 ⟩	⟨ 0.904 ⟩
γ_1 Treat × During Subs	687.911	923.199	235.229	2.062
	[384.005, 1103.756]	[530.966, 1318.746]	[97.241, 395.990]	[0.750, 3.044]
	{ 0.001 }	{ 0.006 }	{ 0.017 }	{ 0.006 }
	⟨ 0.015 ⟩	⟨ 0.009 ⟩	⟨ 0.027 ⟩	⟨ 0.039 ⟩
γ_2 Treat × After Subs	531.499	499.159	–32.340	1.302
	[–144.915, 1079.030]	[–259.119, 1109.044]	[–195.164, 108.545]	[–0.482, 2.647]
	{ 0.252 }	{ 0.281 }	{ 0.494 }	{ 0.285 }
	⟨ 0.319 ⟩	⟨ 0.414 ⟩	⟨ 0.621 ⟩	⟨ 0.269 ⟩
Pre-subsidy intervention market mean	825.121	1167.304	342.183	4.733
Wild Bootstrap p-value: $\gamma_1 = \gamma_2$	0.560	0.227	0.012	0.070
Fisher p-value: $\gamma_1 = \gamma_2$	0.689	0.298	0.010	0.134
Number of Vendors	1011	1011	1011	1011
Number of Observations	32170	32170	32170	32170

Notes: This table replicates Table 2 excluding vendors that frequently buy in the two most popular wholesale markets among treated vendors in the sample, Kole Market and Sealdaha. Relative to our full sample, this sample excludes 716 vendors. Coefficients for During and Post not shown. 95% wild bootstrap confidence intervals are in [], wild bootstrap p-value is in {}, and Fisher permutation p-value is in ⟨⟩. The outcome in column 1 is the total cost of wholesale purchases on a given day, the outcome in column 2 is the vendor's total revenues on a given day accruing from all produce, the outcome in column 3 is the daily profits accrued from all produce, and the outcome in column 4 is the number of distinct types of vegetables a vendor has available on a given day. Profits are calculated by computing (amount of vegetable at the start of the day - amount left over at the end of the day)*anticipated sale price - (amount procured at the start of the day * procurement cost). On days where amount left over was not observed, we impute the vendor's average amount left over all days in which it was measured.